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TRANSACTIONS

OF

THE AMERICAN SOCIETY OF REFRIGERATING ENGINEERS

VOLUME IV.

Fourth Meeting, New York, N. Y., 1908.
November 30 and December 1.

PUBLISHED BY THE SOCIETY
FROM ITS OFFICES, SUITE 1724, NO. 154 NASSAU STREET,
NEW YORK, N. Y.

Sec 1520, 517

Nov. 1, 1912
HARVARD UNIVERSITY
DEPARTMENT OF ENGINEERING
(IV, V)

Jul. 1, 1919
Transferred to
Harvard College Library

Organized December 5, 1904, at New York, N. Y.

Incorporated under the Laws of the State of New York.
August 30, 1905.

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OFFICERS

OF

THE AMERICAN SOCIETY OF REFRIGERATING ENGINEERS.

1907-1908.

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EDGAR PENNEY.....Newburgh, N. Y.

VICE-PRESIDENTS.

JAMES WILLS.....New York, N. Y.
Term expired at Annual Meeting of 1908.

KARL E. VESTERDAHL.....New York, N. Y.
Term expires at Annual Meeting of 1909.

DIRECTORS.

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JOHN E. STARR.....New York, N. Y.
H. B. ROELKER.....New York, N. Y.
Terms expired at Annual Meeting of 1908.

J. J. DE KINDER.....Philadelphia, Pa.
F. W. PILSBRY.....Chicago, Ill.
LLEWELLYN WILLIAMS.....Boston, Mass.
Terms expire at Annual Meeting of 1909.

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TREASURER.

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SECRETARY.

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OF

THE AMERICAN SOCIETY OF REFRIGERATING ENGINEERS.

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VICE-PRESIDENTS.

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Term expired at Annual Meeting of 1907.

JAMES WILLS.....New York, N. Y.
Term expired at Annual Meeting of 1908.

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THOMAS SHIPLEY.....York, Pa.
HENRY TORRANCE, JR.....New York, N. Y.
L. HOWARD JENKS.....New York, N. Y.
Terms expired at Annual Meeting of 1907.

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SECRETARY.

WILLIAM H. ROSS.....New York, N. Y.

OFFICERS

OF

THE AMERICAN SOCIETY OF REFRIGERATING ENGINEERS.

1905-1906.

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VICE-PRESIDENTS.

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Term expired at Annual Meeting of 1906.

JOHN E. STARR.....New York, N. Y.

Term expired at Annual Meeting of 1907.

DIRECTORS.

ELLIS L. PHILLIPS.....New York, N. Y.
D. S. JACOBUS.....Hoboken, N. J.
W. T. ROBINSON.....Philadelphia, Pa.

Terms expired at Annual Meeting of 1906.

HENRY TORRANCE, JR.....New York, N. Y.
THOMAS SHIPLEY.....York, Pa.
L. HOWARD JENKS.....New York, N. Y.

Terms expired at Annual Meeting of 1907.

LOUIS BLOCK.....New York, N. Y.
EDGAR PENNEY.....Newburgh, N. Y.
H. B. ROELKER.....New York, N. Y.

Terms expired at Annual Meeting of 1908.

TREASURER.

WALTER C. REID.....New York, N. Y.

SECRETARY.

WILLIAM H. ROSS.....New York, N. Y.

OFFICERS

OF

THE AMERICAN SOCIETY OF REFRIGERATING ENGINEERS.

1904-1905.

PRESIDENT.

JOHN E. STARR.....New York, N. Y.

VICE-PRESIDENTS.

H. B. ROELKER.....New York, N. Y.
Term expired at Annual Meeting of 1905.

P. D. C. BALL.....St. Louis, Mo.
Term expired at Annual Meeting of 1906.

DIRECTORS.

LOUIS BLOCK.....New York, N. Y.
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Terms expired at Annual Meeting of 1905.

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WALTER C. REID.....New York, N. Y.

SECRETARY.

WILLIAM H. ROSS.....New York, N. Y.

THE AMERICAN SOCIETY OF REFRIGERATING ENGINEERS.

CONSTITUTION.

NAME, OBJECT AND GOVERNMENT.

C 1. The title of this Society shall be "The American Society of Refrigerating Engineers."

C 2. The object of this Society is to promote the Arts and Sciences connected with Refrigerating Engineering.

C 3. The principal means for this purpose shall be the holding of meetings for the reading and discussion of appropriate papers, and for social intercourse; the publication and distribution of its papers and discussions; and the maintenance of a library of data on refrigeration.

C 4. The Society shall be governed by this Constitution, and by By-Laws and Rules in harmony therewith.

C 5. The Society shall be organized as a corporation under the laws of the State of New York.

MEMBERSHIP.

C 6. Persons connected with the Arts and Sciences relating to Refrigerating Engineering may be eligible for admission into the Society.

C 7. The membership of the Society shall consist of Members, Associates, and Juniors. Members and Associates are entitled to vote and to hold office. Juniors shall not be entitled to vote nor to be officers of the Society, but shall be entitled to the other privileges of membership.

C 8. Members and Associates are entitled to vote on all questions before any meeting of the Society, in person or by proxy, given to a voting member in writing. A proxy shall not be valid for a greater time than six months.

C 9. A Member shall be twenty-six years of age or over. He must have been so connected with Refrigerating Engineering as to be competent as a designer or as a constructor, to take responsible charge of work in his branch of Refrigerating Engineering, or he

must have served as a teacher of Refrigerating Engineering for more than five years.

C 10. An Associate shall be twenty-six years of age or over and shall be so connected with Refrigerating Engineering as to be competent to take charge of engineering work, or to co-operate with Refrigerating Engineers.

C 11. A Junior shall be twenty-one years of age or over. He must have had such Refrigerating Engineering experience as will enable him to fill a responsible subordinate position in Refrigerating Engineering work, or he must be a graduate of an engineering school.

C 12. The rights and privileges of every Member, Associate and Junior shall be personal to himself, and shall not be transferable or transmissible by his own act or by operation of law.

ADMISSION.

C 13. All applications for membership as Member, Associate or Junior shall be presented to the Council, which shall consider and act upon each application, assigning each approved applicant to the classification to which, in the judgment of the Council, he is entitled. The name of each candidate thus approved by the Council shall, unless objection is made by the applicant, be submitted to the voting membership for election by means of a letter ballot.

C 14. Associates or Juniors desiring to change their grade of membership shall make application to the Council in the same manner as is required in the case of a new applicant.

C 15. Election to membership shall be by a sealed letter ballot as the By-Laws shall provide. Adverse votes to the number of 4 per cent. of the votes cast shall be required to defeat the election of an applicant.

C 16. Each person elected shall subscribe to this Constitution, and shall pay the initiation fee before he can be entitled to the rights and privileges of membership. If such person does not comply with these requirements within six months after notice of his election he will be deemed to have declined election. The Council may, thereupon, declare his election void.

INITIATION FEES AND DUES.

C 17. The initiation fee for membership shall be as follows:

For Members and Associates, five dollars.

For Juniors, five dollars.

A Junior on promotion to any other grade of membership shall pay an additional fee of five dollars.

The annual dues for membership shall be as follows:

For Members and Associates, ten dollars.

For Juniors, five dollars, for the first six years of their membership, and thereafter the same as for a Member or Associate.

SUSPENSIONS AND EXPULSIONS.

C 18. Any Member, Associate or Junior who shall leave his annual dues unpaid for two years shall, at the discretion of the Council, have his name stricken from the roll of membership and shall cease to have any further rights of membership.

C 19. The Council may refuse to receive the dues of any Member, Associate or Junior who shall have been adjudged by the Council to have violated the Constitution or By-Laws of the Society, or who, in the opinion of the Council, expressed by a two-thirds vote of the entire Council, shall have been guilty of conduct rendering him unfit to continue in its membership; and the Council may expel such person and remove his name from the list of members.

THE COUNCIL.

C 20. The affairs of the Society shall be managed by a Board of Directors chosen from among its Members and Associates, which shall be styled "The Council." The Council shall consist of the President of the Society, who shall be presiding officer, the two Vice-Presidents, Treasurer and nine Members or Associates. Seven Members of the Council shall constitute a quorum for the transaction of business. The Secretary may take part in the deliberations of the Council, but shall not have a vote therein.

C 21. The Council thus constituted shall regulate its own proceedings and shall be the legal Trustee of the Society. All gifts or bequests not designated for a specific purpose shall be invested by the Council and only the income therefrom may be used for current expenses.

C 22. Should a vacancy occur in the Council, or in any elective office except the presidency, through death, resignation or other cause, the Council may elect a Member or Associate to fill the vacancy until the next annual election.

C 23. The Council shall present at the Annual Meeting of the Society a report, verified by the President or Treasurer or by a majority of the members of the Council, showing the whole amount of real and personal property owned by the Society, where located,

and where and how invested, and the amount and nature of the property acquired during the year immediately preceding the date of the report, and the manner of the acquisition; the amount applied, appropriated or expended during the year immediately preceding such date, and the purposes, objects or persons to or for which such applications, appropriations or expenditures have been made; also the names and places of residence of the persons who have been admitted to membership in the Society during the last year, which report shall be filed with the records of the Society, and an abstract thereof shall be entered in the minutes of the proceedings of the Annual Meeting.

C 24. An act of the Council which shall have received the expressed or the implied sanction of the membership at the next subsequent meeting of the Society shall be deemed to be the act of the Society, and shall not afterward be impeached by any member.

C 25. The Council may, by a two-thirds vote of the members present, declare any elective office vacant, on the failure of its incumbent for one year, from inability or otherwise, to attend the Council meetings, or to perform the duties of his office, and shall thereupon appoint a Member or Associate to fill the vacancy until the next Annual Meeting. The said appointment shall not render the appointee ineligible to election to any office.

OFFICERS.

C 26. At each Annual Meeting there shall be elected from among the Members and Associates:

A President to hold office for one year.

Two Vice-Presidents, one to hold office for one year and one to hold office for two years. After the first year one Vice-President to be elected annually for a term of two years.

A Treasurer to hold office for one year.

Nine Members or Associates shall be elected to the Council at the first Annual Meeting, three to hold office for one year, three to hold office for two years, and three to hold office for three years. and at each subsequent Annual Meeting three Members or Associates shall be elected, each to serve three years.

C 27. The election of officers shall be by ballot, as the By-Laws shall provide.

C 28. The term of all elective officers shall begin on the adjournment of the Annual Meeting of the Society. Officers shall continue in their respective offices until their successors have been installed.

C 29. A President or Vice-President shall not be eligible for immediate re-election to the same office at the expiration of the term for which he was elected.

C 30. The Council, at its first meeting after the Annual Meeting of the Society, shall appoint a Member or Associate to serve as Secretary of the Society subject to the pleasure of the Council. The Secretary shall receive a salary which shall be fixed by the Council at the time of his appointment.

C 31. The President, Secretary and Treasurer shall perform the duties legally or customarily attaching to their respective offices under the laws of the State of New York, and such other duties as may be required of them by the Council.

C 32. A vacancy in the office of President shall be filled by the Vice-President who is senior by age.

MEETINGS.

C 33. The Society shall hold its Annual Meeting in New York City on the Monday preceding the first Tuesday in December, and such other meetings shall be held at such times and places as the Council may appoint. Twenty-five Members and Associates shall constitute a quorum for the transaction of business.

C 34. Special meetings of the Society may be called at any time at the discretion of the Council or shall be called by the President upon the written request of twenty-five members entitled to vote.

C 35. Any appropriation recommended by the Society at a meeting shall not take effect until it has been approved by the Council.

C 36. Every question which shall come before a meeting of the Society or of the Council or a Committee shall be decided by a majority of the votes cast, unless otherwise provided in this Constitution or the By-Laws, or the laws of the State of New York. The Council may order the submission of any question to the membership for discussion by letter ballot. Any meeting of the Society at which a quorum is present may order the submission of any question to the membership for discussion by letter ballot.

STANDING COMMITTEES.

C 37. The standing committees of the Society shall be:

Finance Committee.

Publication Committee.

Membership Committee.

The members of these Committees shall be appointed by the

President from members of the Council who are not officers of the Society.

TRANSACTIONS.

C 38. The Society shall not be responsible for statements or opinions advanced in papers or in discussions at its meetings. Matters relating to politics, religion or purely to trade shall not be discussed at a meeting of the Society nor be included in the transactions.

C 39. The Society shall not approve any engineering or commercial enterprise, nor allow its imprint or name to be used in any commercial work or business. No member shall describe himself in connection with the Society in any advertisement other than as a Member, Associate Member or Junior Member.

AMENDMENTS.

C 40. At Annual Meetings of the Society any Member or Associate may propose in writing for discussion an amendment to this Constitution. Such proposed amendment shall not be voted on at that meeting, but shall be open for discussion and such modification as may be accepted by the proposer. The proposed amendment shall be mailed by the Secretary to each Member and Associate at the time the notice of the next Annual Meeting issues, and shall be voted upon at said meeting.

C 41. Such By-Laws shall be enacted as will conform with this Constitution and the Laws of the State of New York and as are required to conduct the business of the Society.

BY-LAWS.

CANDIDATES FOR MEMBERSHIP.

B 1. A candidate for admission to the Society as a Member or as an Associate must make application on a form approved by the Council, upon which he shall write a statement giving a complete account of his qualifications and engineering experience, and an agreement that he will, if elected, conform to the Constitution, By-Laws and Rules of the Society. He must refer to at least four Members or Associates to whom he is personally known.

B 2. Applications for membership from Refrigerating Engineers who are not residents of the United States or Canada and who may be so situated as not to be personally known to four Members or Associates of the Society, as required in the foregoing paragraph, may be recommended for ballot by four members of the

Council, after sufficient evidence has been secured to show that in their opinion the applicant is worthy of admission to the grade which he seeks.

B 3. A candidate for admission to the Society as a Junior must make application in the same manner as provided for Members or Associates, except that he must refer to not less than three Members or Associates to whom he is personally known.

B 4. The references for each candidate for admission to the Society shall be requested to make a confidential communication to the Membership Committee, setting forth in detail such information, personally known to the referee, as shall enable the Council to arrive at a proper estimate of the eligibility of the candidate for admission to the Society.

ELECTION OF MEMBERS.

B 5. The Secretary shall mail to each member entitled to vote, at least thirty days in advance of any meeting, a ballot stating the names and the respective grades of the candidates for membership in the Society who have been approved by the Council, and the time of the closure of voting. The voter shall prepare his ballot by crossing out the names of candidates rejected by him, and shall enclose said ballot in a sealed blank ballot envelope, which he shall then enclose in a second sealed outer envelope on which he shall, for identification, write his name in ink. The ballot thus prepared and enclosed shall be mailed or delivered unopened to the Tellers of Election. The Secretary shall certify to the competency and the signature of all voters. On the closure of voting, the Tellers of Election shall first open and destroy the outer envelopes, and shall then canvass the ballots, and certify the result to the meeting of the Society.

B 6. The Tellers of Election shall not receive any ballot after the stated time of the closure of voting. A ballot without the endorsement of the voter written in ink on the outer envelope is defective, and shall be rejected by the Tellers of Election.

B 7. The names of those persons elected to membership, with their respective grades, shall be embodied in a written report, signed by the Tellers, and presented to the next meeting of the Society. The President shall then declare them duly elected to membership in the Society. The Tellers may, through the Secretary, in advance of any meeting, advise each candidate of the result of the canvass of the votes in his case. The names of applicants who are not elected shall neither be announced nor recorded in the Transactions.

B 8. The endorsers of an applicant who has not been elected may, with his consent, present to the Council a written request for a re-submission of his name to ballot. The Council may, in its discretion, by a three-fourths vote of the members present, order the name of the applicant placed on the next ballot for members.

B 9. Each person elected to membership must subscribe to the Constitution, By-Laws and Rules of the Society, and pay the initiation fee before he can receive a certificate of membership in the Society.

ELECTION OF OFFICERS.

B 10. The Secretary shall mail to each member entitled to vote, at least thirty days before the Annual Meeting, the names of the candidates for office proposed for election by the Nominating Committees.

B 11. The names of the candidates proposed by the Nominating Committee or Committees, and the respective offices for which they are candidates, shall be printed in separate lists on the same ballot sheet, each list of candidates to be printed under the names of the members of the particular committee which proposed it.

B 12. The name of any candidate on the ballot may be erased, and the name of any person qualified to hold the office written in its stead. The ballot must be voted and canvassed in the same manner as for the election of members.

B 13. In case of a tie in the vote for any officer, the President, or, in his absence, the Presiding Officer, shall cast the deciding vote.

B 14. A ballot which contains more names on it than there are officers to be elected is thereby defective, and shall be rejected by the Tellers.

FEES AND DUES.

B 15. The initiation fee and annual dues of the first year shall be due and payable on notice of election to membership. Thereafter the annual dues shall be due and payable on the first day of December in each year.

B 16. A member in arrears for one year shall not be entitled to vote until such arrears have been paid. Should the right to vote be questioned, the books of the Society shall be conclusive evidence.

B 17. The Secretary shall present to the Council the name of any Member, Associate or Junior in arrears for more than one year. A person dropped from the rolls for non-payment of dues may, at the discretion of the Council, be restored to the privileges of membership upon payment of all arrears.

FINANCIAL ADMINISTRATION.

B 18. The Council at its first meeting in each fiscal year shall consider the recommendations of the Finance Committee concerning the expenditures necessary for the work of the Society during that year. The apportioning of the work of the Society among the various standing and other Committees shall be on a basis approved by the Council and in harmony with the Constitution and By-Laws. The appropriations approved by the Council, or so much thereof as may be required for the work of the Society, shall be expended by the various Committees of the Society, and all bills against the Society for such expenditure shall be certified by the Committee making the expenditure and shall then be sent to the Finance Committee for audit. Money shall not be paid out by any officer or employee of the Society except upon bills duly audited by the Committee, or by resolution of the Council.

FINANCE COMMITTEE.

B 19. The Finance Committee shall consist of three members of the Council appointed for a term of one year. The Committee shall, under the direction of the Council, have supervision of the financial affairs of the Society, including the books of account.

PROGRAM COMMITTEE.

B 20. The President shall appoint a committee from the members of the Council who shall procure professional papers, to pass upon their suitability for presentation, and to suggest topical subjects for discussion at the meetings. The Committee may refer any paper presented to the Society to a person or persons, especially qualified by theoretical knowledge or practical experience, for their suggestions or opinions as to the suitability of the paper for presentation. Papers from non-members shall not be accepted except by unanimous vote of the Committee. The Committee shall arrange the program of each meeting of the Society, and shall have general charge of the entertainments to be provided for the members and guests at each meeting. It shall prohibit the distribution or exhibition at the headquarters or at the meeting places of the Society of all advertising circulars, pamphlets or samples of commercial apparatus or machinery. At the end of each fiscal year the Committee shall deliver to the Secretary for presentation to the Council a detailed report of its work.

PUBLICATION COMMITTEE.

B 21. The Publication Committee shall consist of three members of the Council appointed for a term of one year. The Com-

mittee shall review all papers and discussions which have been presented at the meetings, and shall decide what papers or discussions, or parts of the same, shall be printed. At the end of each fiscal year the Committee shall deliver to the Secretary for presentation to the Council a detailed report of its work.

MEMBERSHIP COMMITTEE.

B 22. The Membership Committee shall consist of three members of the Council appointed for a term of one year. It shall be the duty of this Committee:

To receive and scrutinize all applications for membership to the Society.

To seek further information as to the qualifications of any applicant whose evidence of eligibility is not clear to the Committee.

To report to each session of the Council the names of all applicants under consideration, together with the action of the Committee on each.

The Committee shall at once destroy all correspondence in relation to each applicant when his name has been placed on the ballot by order of the Council, or upon the withdrawal of the application.

NOMINATING COMMITTEES.

B 23. A Nominating Committee of five Members or Associates shall be appointed by the President within three months after he assumes office. It shall be the duty of this Committee to send to the Secretary on or before October 1st the names of consenting nominees for the elective offices next falling vacant under the Constitution. Upon the request of any Member or Associate, the Secretary shall furnish to the applicant the names of such nominees.

B 24. A special Nominating Committee, if organized, shall, on or before October 20th, present to the Secretary the names of the candidates nominated by it for the elective offices next falling vacant under the Constitution, together with the written consent of each.

TELLERS.

B 25. The Presiding Officer shall, at the first session of the Annual Meeting, appoint three Tellers of Election of Officers, whose duties shall be to canvass the votes cast, and report the result to the meeting. Their term of office shall terminate when their report of the canvass is presented to the meeting.

B 26. The President within one month after assuming office shall appoint three Tellers of Election of Members to serve for one

year, whose duties shall be to canvass the votes cast for members during the year, and to certify the same to the President. They shall notify candidates through the Secretary of the result of such election.

B 27. The President shall appoint three Tellers to canvass any letter ballots which shall be ordered by the Council or by the Society.

SECRETARY.

B 28. The Secretary of the Society shall be the Secretary to the Council.

The Secretary shall, under the supervision of the Finance Committee, have charge of the Books of Account of the Society.

He shall make and collect all bills against members or others.

All bills against the Society shall be delivered to the Secretary. He shall immediately enter them in the Books of Account, and shall immediately deposit such funds as he receives to the credit of the Society in a bank to be designated by the Council.

TREASURER.

B 29. The Treasurer shall make payments only on the audit of the Finance Committee, or upon the direction of the Council, by resolution of that body. He shall furnish a bond for the faithful performance of his duties to such amount as the Council may require, such bond to be procured from an incorporated Guarantee Company, at the expense of the Society.

TITLES, EMBLEMS, CERTIFICATES.

B 30. Each Member, Associate and Junior shall, subject to such rules as the Council may establish, be entitled on request to a certificate of membership, signed by the President and Secretary of the Society. Every such certificate shall remain the property of the Society, and shall be returned to it on demand of the Council.

B 31. Each proxy authorizing a person to vote for an absent member shall be signed by such absent member, with an attesting witness, and be submitted to the Secretary for verification of the member's right to vote at the meeting at which the right is to be exercised.

B 32. The emblem of each grade of membership approved by the Council shall be worn by those only who belong to that grade. The official stationery shall be used only by Officers and Committees of the Society for official business.

B 34. The abbreviations of the titles of the various grades of membership approved by the Society are as follows:

For Members—Mem. Am. Soc. R. E.
For Associates—Assoc. Am. Soc. R. E.
For Juniors—Jun. Am. Soc. R. E.

ORDER OF BUSINESS.

B 34. Roll Call.
Reading of minutes of previous meeting.
Report of Tellers of Election of Membership.
Report of the Council.
Unfinished business.
New business.
Report of Tellers of Election of Officers.
Reading of papers and discussions.

PARLIAMENTARY RULES.

B 35. In all questions arising at any meeting, involving parliamentary rules not provided for in these By-Laws, "Cushing's Manual" shall be the governing authority.

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PROCEEDINGS

OF THE

FOURTH ANNUAL MEETING

OF

THE AMERICAN SOCIETY OF REFRIGERATING ENGINEERS

New York, N. Y., November 30 and December 1, 1908.

The American Society of Refrigerating Engineers held its fourth annual meeting in the Engineering Societies Building, No. 29 West Thirty-ninth street, New York, N. Y., on Monday and Tuesday, November 20, and December 1, 1908. Five sessions were held, Monday morning, afternoon and evening, and Tuesday morning and afternoon, all being called to order and presided over by President Edgar Penney.

First Session, Monday Morning, November 30.

The first session of the meeting was called to order at about 10:30 o'clock A. M., and the following members responded to the roll call:

Berna, George.....Ithaca, N. Y.
Birkholz, Julius W.New York, N. Y.
Bishop, A. C.....Detroit, Mich.
Block, Louis.....New York, N. Y.
Borgstedt, Henning N.,
Yonkers, N. Y.
Briggs, John N....Coeymans, N. Y.
Brownell, C. H.,
Washington Court House, Ohio
Burhorn, Edwin...New York, N. Y.
Cole, Harold W...New York, N. Y.
Cooper, Madison..Watertown, N. Y.
Dickerman, Charles.Philadelphia, Pa.
Ely, James.....New York, N. Y.
Frantz, Samuel R..Waynesboro, Pa.
Friedmann, Edward N.,
New York, N. Y.
Greene, Van Rensselaer H.,
New York, N. Y.
Guckel, Charles H.,
Colon, Rep. of Panama
Gueth, Oswald.....Brooklyn, N. Y.
Haire, David E.....Philadelphia, Pa.
Hargreaves, William,
New York, N. Y.
Herst, Franklin H..Philadelphia, Pa.
Hill, Walter L.....Boston, Mass.
Hiller, N. H.....Carbondale, Pa.
Holden, D. L.....New York, N. Y.
Horne, Frank A..New York, N. Y.
Jacobus, D. S.....New York, N. Y.
Jenks, L. Howard.New York, N. Y.
Johnson, Albert.....St. Louis, Mo.
Kolischer, T.....Philadelphia, Pa.
Lewis, Eugene W...Cincinnati, Ohio
Livezey, John R....Philadelphia, Pa.
McCarthy, George L.,
New York, N. Y.

McDaniel, Homer...Cleveland, Ohio
Manns, W. H.....Waynesboro, Pa.
Markel, E. J.....Hartford, Conn.
Matthews, F. E...New York, N. Y.
Palmer, C. C.....New York, N. Y.
Parsons, Charles H., Springfield, Mass.
Penney, Edgar.....Newburgh, N. Y.
Pilsbry, F. W.....Chicago, Ill.
Ross, W. H.....New York, N. Y.
Sargent, Allston...New York, N. Y.
Shipley, Samuel J..New York, N. Y.
Shipley, Thomas.....York, Pa.
Shipley, William S..New York, N. Y.
Shipman, R. L.....Ithaca, N. Y.
Smith, Guy B.....Brooklyn, N. Y.
Sparks, John C....New York, N. Y.
Starr, John E.....New York, N. Y.
Stevenson, S. P.....Chester, Pa.
Stone, Junius H...New York, N. Y.
Strickler, A. B.....York, Pa.
Tinker, Henry W.....Boston, Mass.
Torrance, Henry, Jr., New York, N. Y.
Vesterdahl, Karl..New York, N. Y.
Voorhees, G. T.....Boston, Mass.
Waite, Nelson J.....Cleveland, Ohio
Walter, Bruce.....Pittsburg, Pa.
Warner, Irving....Wilmington, Del.
Wegemann, Karl...New York, N. Y.
Werliin, L. .....Philadelphia, Pa.
Winborg, Abel.....York, Pa.
Woodcock, Willard J., Brooklyn, N. Y.
Wright, George W...Baltimore, Md.

The minutes of the third annual meeting were then read and approved. They were as follows:

MINUTES OF THE THIRD ANNUAL MEETING OF THE AMERICAN SOCIETY OF REFRIGERATING ENGINEERS.

The third annual meeting of The American Society of Refrigerating Engineers was held in the Engineering Societies Building, No. 29 West Thirty-ninth street, New York, N. Y., on Monday and Tuesday, December 2 and 3, 1907. Four sessions were held, Monday morning, afternoon and evening, and Tuesday morning. Owing to the unavoidable absence of President D. S. Jacobus, the sessions were presided over by Vice-Presidents John E. Starr and James Wills. Vice-President Starr called the sessions of Monday morning and afternoon and Tuesday morning to order, while Vice-President Wills presided over Monday evening's session.

FIRST SESSION, MONDAY MORNING, DECEMBER 2.

The first session of the meeting was called to order by Vice-President John E. Starr at about 10 A. M., and he announced that President D. S. Jacobus was unable to attend the meeting, he being in California on some important engineering work that required his personal attention, and that the sessions would be presided over by the Vice-Presidents.

The following letter from President Jacobus was read at this point:

HOTEL REDONDO,
REDONDO BEACH, Cal., November 17th, 1907.

Dear Mr. Ross:

I have waited until the last moment before sending you word that I will not be able to attend the coming meeting of The American Society of Refrigerating Engineers. I hoped that it would be possible for me to be back in time, or to make a special trip East for the meeting and return to work here, as I did last year when at Denver, but circumstances are such that it will be impossible for me to do this. Kindly tender my regrets to those at the meeting, and say that I cannot express my great disappointment in not being able to be with them.

Enclosed you will find a copy of an address which I prepared for pre-

sentation at the meeting. Will you read this for me and extend to all my best greetings?

With earnest wishes for the continued prosperity of the Society, I am,

Yours very truly, D. S. JACOBUS.

To Mr. W. H. Ross, Secretary,
The American Society of Refrigerating Engineers,
No. 258 Broadway, New York City.

The roll call of members was now heard and forty-three were found to be present.

Owing to the absence of President Jacobus the committee that was to conduct tests at the experimental plant of the York Manufacturing Company, at York, Pa., made no report and was continued. Professor R. L. Shipman, of Cornell University, stated that as the test plant of the York Manufacturing Company had been dismantled, he took pleasure in inviting the committee to conduct its tests on the new experimental plant at Sibley College of Cornell University. Professor Shipman was thanked for his invitation, and the matter was left to the committee.

By unanimous vote the Secretary was instructed to send the following telegram of greeting to President Jacobus:

NEW YORK, N. Y., December 2, 1907.

DR. D. S. JACOBUS, Redondo Beach, Cal.:

The American Society of Refrigerating Engineers in convention assembled extends greetings to its president and regrets his unavoidable absence.

W. H. ROSS, Secretary.

At this point Vice-President Starr suggested that the Society should take some cognizance of the first meeting of the International Congress of the Refrigerating Industries to be held in Paris, France, in 1908, and stated that a meeting of representatives of various organizations, including The American Society of Refrigerating Engineers, by authority of the Council, was held in Buffalo on November 14, and perfected the organization of an American committee to take charge of the work in this country. He stated that it was desired that all organizations in any way connected with refrigeration should co-operate with the American committee, and if possible assist it in a financial way to enable it to carry on the work expected of the committee, and also pass resolutions asking our National Government to recognize the Congress in an official way and urge the acceptance of the invitation of the French Government to participate in the Congress by sending accredited representatives. He further stated that the Council had power to take the necessary action, but it was felt that an expression of the membership on the subject was desired.

The following resolution was offered and, after discussion, accepted:

RESOLUTION.

WHEREAS, An International Congress of the Refrigerating Industries, to be held under the auspices of the French Government, has been called to convene in Paris, France, in 1908; and

WHEREAS, The American Society of Refrigerating Engineers and the trade organizations have been invited to co-operate and participate in said Congress; therefore be it

Resolved, That The American Society of Refrigerating Engineers, in convention assembled, recognizing the importance of such International Congress, does hereby heartily endorse the holding of said Congress, and urges the membership of the Society to become members of this Congress, to the end that the maximum benefits from such an International gathering may be secured to our Society; therefore be it further

Resolved, That the Government of the United States, through its proper officials, is hereby requested and urged to give due and adequate recognition of this first International Congress of the Refrigerating Industries, and to accept the invitation of the French Government to participate in its proceedings by sending duly accredited representatives of the United States Government to attend the Congress.

The following motion was introduced by L. Howard Jenks, and after being discussed was accepted:

I move that this convention recommend to the Council of the Society that an appropriation be made to the American Committee of the International Congress of the Refrigerating Industries, that we may co-operate in the movement to develop the arts and sciences in relation to refrigerating engineering, for which purpose this Society was formed.

The Secretary then proposed an amendment to paragraph B 24 of the By-Laws for discussion at this, the third annual meeting, and for further discussion and final action at the next, the fourth annual meeting.

Paragraph B 24 now reads:

B 24. A Special Nominating Committee, if organized, shall on or before October 20th present to the Secretary the names of the candidates nominated by it for the elective offices next falling vacant under the Constitution, together with the written consent of each.

He would have it read as follows:

B 24. Twenty or more members entitled to vote may constitute themselves a Special Nominating Committee with the same powers as the Annual Nominating Committee. A Special Nominating Committee, if organized, shall, on or before October 20th, present to the Secretary the names of the candidates nominated by it for the elective offices next falling vacant under the Constitution, together with the written consent of each.

He further stated that when the committee appointed to draft a Constitution and By-Laws made its report, it overlooked the insertion in the By-Laws of a paragraph defining the number of members to constitute a Special Nominating Committee, and its powers, and stated that he felt that if this amendment was accepted it would cover this oversight.

He further proposed an amendment to the By-Laws by adding a paragraph, to be known as paragraph B 36, covering the manner of amending the By-Laws. It was as follows:

B 36. These By-Laws may be amended in the same manner as amendments are made to the Constitution.

The Tellers of Election of Officers now made their report. It was as follows:

REPORT OF THE TELLERS OF ELECTION OF OFFICERS.

NEW YORK, N. Y., December 2, 1907.

Dr. D. S. JACOBUS, President,
The American Society of Refrigerating Engineers,
No. 258 Broadway, New York, N. Y.

Dear Sir:

Your Tellers of Election of Officers have canvassed the ballots and beg to report the election of the following gentlemen:

President—Edgar Penney.
Vice-President—Karl E. Vesterdahl.
Treasurer—Walter C. Reid.
Directors { Thomas Shipley. John C. Wait. Karl Wegemann.

Respectively submitted,

(Signed) ALBERT JOHNSON, A. B. STRICKLER, G. T. VOORHEES, } *Tellers of Election of Officers.*

Chairman Starr appointed Albert Johnson and Harold W. Cole as a committee to escort the President-elect to a chair beside him.

President-elect Penney, in accepting the presidency, stated as follows:

"I appreciate highly the honor imposed on me, and trust with your co-operation and assistance that I may be able to fulfill the duties. Now don't be backward in helping all you can as opportunity presents itself."

Chairman Starr now read President Jacobus' address. It was heartily received.

The session then adjourned for luncheon in the building, arranged for by the Program Committee.

SECOND SESSION, MONDAY AFTERNOON, DECEMBER 2.

The second session of the meeting was called to order at about 1:30 P. M. The following three papers were presented at this session:

"Cold Storage by Means of Ice," by Madison Cooper; "Recent Investigations in the Handling of Perishable Products for Transportation by the United States Department of Agriculture," by S. J. Dennis; "The Automatic Refrigerating Machine," by Van Rensselaer H. Greene.

Mr. Dennis prefaced his paper with the following remarks:

"Before proceeding to read my paper I should perhaps add a word of explanation as to the reason why I have limited myself to rather general statements, and that is for the fact that this same matter is in process of publication by the United States Department of Agriculture, and it is not the policy of the Department to issue for publication matter which is about to be published."

A vote of thanks was extended Mr. Dennis for his able and interesting paper.

The session then adjourned.

THIRD SESSION, MONDAY EVENING, DECEMBER 2.

The third session of the meeting was called to order shortly after 8 P. M. The following telegram from President Jacobus was read:

REDONDO, Cal., December 2, 1907.

Mr. JOHN E. STARR, Chairman,
The American Society of Refrigerating Engineers,
No. 29 West Thirty-ninth street,
New York, N. Y.

Convey to the members my deep regret in not being able to be present. This is indeed the greatest of disappointments, and I certainly would have attended had it been possible. I will be with you in spirit, and wish the Society a most successful meeting. D. S. JACOBUS.

The reading and discussion of papers was now taken up. The papers read at this session were:

"Experiences in the Manufacture of Plate Ice," by Irving Warner; "Fireproof Cold Storage Warehouse Construction," by John E. Starr; "The Best Method of Detecting Impurities in Ammonia in Refrigerating Plants," by John C. Sparks.

The session then adjourned.

FOURTH SESSION, TUESDAY MORNING, DECEMBER 3.

The last session of the meeting was called to order shortly after 9 A. M. The papers presented at this session were as follows:

"Comparison of Bids for Ice-Making and Refrigerating Machinery," by Thomas Shipley; "The Work of the United States Geological Survey in

Its Relation to Refrigeration," by R. B. Dole; "Advantages of the Wet Compression System," by Carl W. Vollmann; "Heat Transfer in Coolers and Condensers of the Double-Pipe Type," by R. L. Shipman.

A vote of thanks was extended to Mr. Dole for his valuable paper, and in responding Mr. Dole said:

"I thank you in the name of the Geological Survey and the other members of the branch, who have worked, probably all of them, harder than I to collect this information. I should like to add something that I forgot to say, namely, that these water-supply papers, written in popular style, are printed for practical use, and a certain percentage of each edition is set aside for free distribution. Copies can be obtained by application to the Director of the Geological Survey. Some of them are now out of stock, but those that are marked out of stock for free distribution can generally be obtained from the Superintendent of Public Documents at Washington by the payment of 5 to 10 cents, covering the actual cost of printing."

The topical discussions, "The Standardizing of Measuring Instruments," also received the attention of this session.

A vote of thanks was extended to Vice-President Starr for the very efficient and very pleasing manner in which he occupied the chair in the absence of President Jacobus.

The meeting then adjourned.

(Signed) W. H. Ross, Secretary.

The next business to receive the attention of the meeting was the report of the Tellers of Election of Membership. It was as follows:

NEW YORK, N. Y., November 19, 1908.

Mr. EDGAR PENNEY, President,
The American Society of Refrigerating Engineers,
No. 154 Nassau street,
New York, N. Y.

Dear Sir:

Your Tellers of Election of Members met this day and canvassed the ballots for new members, and beg to report that the following gentlemen have been duly elected members of the Society in the grade indicated during the year:

MEMBERS.

Dettmar, Ernst....New York, N. Y.
Doelling, Louis Karl, New York, N. Y.
Havenstrite, Charles David, Newark, N. J.
Huntley, Clifford E., New York, N. Y.
Lucke, Charles Edward, New York, N. Y.
Mayer, Fred W...Jersey City, N. J.
Neubecker, Jean...New York, N. Y.
Nolde, Fred.....New York, N. Y.
Riley, William Herman, Wilmington, Del.
Stratton, Harrison D., Philadelphia, Pa.
Uchimura, Tatsujiro...Tokio, Japan
Vilter, Theodore O., Milwaukee, Wis.
Wuertenbaecher, John J., St. Louis, Mo.

ASSOCIATE MEMBERS.

Abbey, Frank Humphrey, Hudson, N. Y.
Brackeu, John Henry, Winona, Minn.
Golden, Albert J...Philadelphia, Pa.
Hexamer, William E., Philadelphia, Pa.
Hier, Frank Taylor.Cincinnati, Ohio

JUNIOR MEMBERS.

Jaeger, Anton Paolo...Chicago, Ill.
Jamison, J. V., Jr..Hagerstown, Md.
List, Leonardo J.....Camden, N. J.

Eighty-five ballots were cast, but two were void, one of these two being received after the closure of voting, and the other did not contain the signature of the voter on the outer envelope.

Respectively submitted,

(Signed) E. FRIEDMANN, Chairman, FRANK A. HORNE, JAMES ELY, *Tellers of Election of Membership.*

The President declared the successful applicants for membership duly elected to their respective grades, provided they had complied with the provisions of the Constitution and By-Laws relating to new members, and requested the Secretary to call the roll of new members. The following responded:

Dettmar, E.....New York, N. Y.
Havenstrite, Charles D.Newark, N. J.
Hexamer, William E., Philadelphia, Pa.
Jamison, J. V., Jr..Hagerstown, Md.
Lucke, Charles E..New York, N. Y.
Mayer, Fred W..Jersey City, N. J.
Neubecker, Jean..New York, N. Y.
Nolde, Fred.....New York, N. Y.
Riley, William H..Wilmington, Del.
Stratton, Harrison D., Philadelphia, Pa.

The report of the Council was next heard and accepted. It was as follows:

REPORT OF THE COUNCIL OF THE AMERICAN SOCIETY OF REFRIGERATING ENGINEERS FOR THE FISCAL YEAR ENDING NOVEMBER 30, 1908.

The amount of real and personal property owned by the Society, the property acquired during the year, the amount applied, appropriated or expended, and the purposes and objects for which such expenditures have been made are contained in the report of the Finance Committee, which follows:

NEW YORK, N. Y., November 22, 1908.

Mr. EDGAR PENNEY, President
The American Society of Refrigerating Engineers,
No. 154 Nassau street,
New York, N. Y.

Dear Sir:

Your Finance Committee has to report as follows for the fiscal year ending November 28, 1908:

Balance on hand December 2, 1907		$615.63

RECEIPTS.

Initiation fees	$185.00	
Dues	1,570.75	
Emblems	44.00	
Transactions	60.00	
Banquet	186.10	
		$2,045.85
Grand Total		$2,661.48

DISBURSEMENTS.

Printing	$683.78	
Emblems	58.50	
Rents	119.16	
Caterers	293.40	
Membership Certificates	38.25	
Furniture	102.42	
Membership in International Congress of the Refrigerating Industries	100.00	
Premium on Treasurer's Bond	5.00	
Salaries	625.00	
Stenographic Report of 1907 Meeting	112.50	
Petty Cash	226.34	
		$2,364.35
Balance on hand		$297.13

Respectfully submitted,
(Signed) THOMAS SHIPLEY, Chairman.

The Council held two executive sessions during 1908, your President presiding on both occasions, and it respectfully presents herewith a report of the business transacted, along with other matters that should interest the membership.

At the first meeting of the Council, on June 3, ex-President John E. Starr was elected a Director to fill the unexpired term of President Penney.

William H. Ross was reappointed Secretary of the Society at the first meeting of the Council. An appropriation of $600 was made for the salary of the Secretary and for the services of an office stenographer.

President Penney made the following committee appointments on June 3:

MEMBERSHIP COMMITTEE.

Louis Block, Chairman.

John E. Starr. J. J. de Kinder.

FINANCE COMMITTEE.

Thomas Shipley, Chairman.

H. B. Roelker. John C. Wait.

PUBLICATION COMMITTEE.

Karl Wegemann, Chairman.

F. W. Pilsbry. L. Williams.

PROGRAM COMMITTEE.

Karl E. Vesterdahl, Chairman.

John E. Starr. James Wills.

NOMINATING COMMITTEE.

D. S. Jacobus, Chairman.

L. Howard Jenks. Henry Torrance, Jr.
F. E. Matthews. Junius H. Stone.

TELLERS OF ELECTION OF MEMBERSHIP.

Edward N. Friedmann, Chairman.

Frank A. Horne. James Ely.

The Council is having 500 copies of the 1907 Transactions printed. They will be sent to the members in a fortnight. The Council had 1,000 copies of the 1908 Year Book printed and copies have been sent to the members.

An appropriation of $100 was made to the American Committee of the International Congress of the Refrigerating Industries for a Donating Membership in the Congress.

Your President was authorized to appoint a delegate and three alternate delegates to the International Congress of the Refrigerating Industries, which was held in Paris, France, on October 10 to 17. After consulting with the members of the Council he appointed ex-President John E. Starr as the delegate and Director Louis Block, Director H. B. Roelker and himself as the alternate delegates.

Of the ten delegates appointed by Secretary of State Root officially to represent the United States at the International Congress of the Refrigerating Industries seven were members of this Society. They were John E. Starr, J. F. Nickerson, Gardner T. Voorhees, Theodore Kolischer, Henry W. Tinker, your President and your Secretary.

The Council cabled the greetings of the Society to the first meeting of the International Congress of the Refrigerating Industries, and, acting jointly with the Council of the American Society of Mechanical Engineers, invited the Congress to hold its second meeting in the United States and extended the use of the Engineering Societies Building for the sessions of the Congress.

The following resolutions were passed on the death of Frank Herbert White:

FRANK HERBERT WHITE.

WHEREAS, The inscrutable workings of a Divine Providence have cut off, in the midst of his career, Frank Herbert White, a member of The American Society of Refrigerating Engineers; and

WHEREAS, He was an ornament to his profession, a loyal and active member of the Society, therefore be it

Resolved, That the membership of The American Society of Refrigerating Engineers hereby record their appreciation of his services to the science of refrigeration and to the Society and their deep sense of personal loss in his death, and be it furthermore

Resolved, That these resolutions be entered upon the record of the Society and that a copy of them be transmitted to his family with their heartfelt sympathy.

Your President and Secretary were appointed a committee to secure permanent offices for the Society and they selected Suite 1,724 in the Tribune Building at No. 154 Nassau Street, New York, N. Y., for quarters of the Society, where all members are welcome and which out of town members may use as their headquarters when in the city.

An appropriation not to exceed $300 was made to cover the rent for the offices for one year. An additional appropriation of $75 was made for office furniture.

The exchange of our Transactions for copies of the periodical "Ice," Atlanta, Ga., has been authorized, also the exchange of our Transactions for the publications of the Société Industrielle de L'Est, Nancy, France.

The resignation of W. T. Robinson as an Associate Member of the Society has been accepted with regret. Mr. Robinson's services and good counsel have been of much value to the Society, especially during the years he served as a Director.

At the first meeting of the Council your President was instructed to appoint a committee of three to secure a gavel for the use of the presidents, and he named Vice-President James Wills, Director Louis Block and the Secretary on the committee. The committee presented the gavel at the second meeting of the Council and it was discharged.

Director John C. Wait, Vice-President James Wills, Director Louis Block and the Secretary were appointed a committee at the first meeting of the Council to draft resolutions on the death of John Glenn Collins, who died at his home in Chicago, Ill., on April 19, 1908. The committee reported the following resolutions at the second meeting of the Council and they were accepted:

JOHN GLENN COLLINS.

WHEREAS, It has pleased Almighty God to remove from this life, full of honors, John Glenn Collins, a loyal member of The American Society of Refrigerating Engineers, and

WHEREAS, His valued services to the field of mechanical refrigeration have given him a world wide reputation and have reflected honor upon The American Society of Refrigerating Engineers, therefore be it

Resolved, That the membership of The American Society of Refrigerating Engineers hereby express their sense of the great loss which they have sustained by his death and their appreciation of the services he has rendered, and be it furthermore

Resolved, That these resolutions be entered upon the minutes of the Society and that a copy of them be transmitted to his family and business associates, with their deep sympathy.

Ex-President D. S. Jacobus was a guest of the Council at its second meeting and suggested the appointing of a committee of five to act jointly with a like committee of The American Society of Mechanical Engineers to make a joint report on establishing a code of rules for testing refrigerating apparatus and also a tonnage basis of refrigeration. The subject was thoroughly discussed and the following resolution passed:

RESOLUTION.

Resolved, That the Council of The American Society of Refrigerating Engineers at a meeting held October 14, 1908, at the recommendation of Ex-President Jacobus, appoint a committee of five members to act jointly with a similar committee of The American Society of Mechanical Engineers to establish a tonnage basis of refrigeration and a code of rules for testing refrigerating apparatus, said committee to be appointed by the President.

On November 2 your President appointed Director John E. Starr, Director Louis Block, F. E. Matthews, Director Thomas Shipley and Ex-President W. Everett Parsons on the joint committee.

The following have been elected to membership in the Society in the three grades during the year:

MEMBERS.

Dettmar, Ernst.....New York, N. Y.
Doelling, Louis K..New York, N. Y.
Havenstrite, Charles D.Newark, N. J.
Huntley, Clifford E.New York, N. Y.
Lucke, Charles E..New York, N. Y.
Mayer, Fred W....Jersey City, N. J.
Neubecker, Jean...New York, N. Y.
Nolde, Fred.....New York, N. Y.
Riley, William H...Wilmington, Del.
Stratton, Harrison D., Philadelphia, Pa.
Uchimura, Tatsujiro...Tokio, Japan
Vilter, Theodore O..Milwaukee, Wis.
Wuertenbaecher, John Jacob, St. Louis, Mo.

ASSOCIATE MEMBERS.

Abbey, Frank H.....Hudson, N. Y.
Bracken, John H.....Winona, Minn.
Golden, Albert J...Philadelphia, Pa.
Hexamer, William E., Philadelphia, Pa.
Hier, Frank T.....Cincinnati, Ohio

JUNIOR MEMBERS.

Jaeger, Anton P.....Chicago, Ill.
Jamison, J. V., Jr..Hagerstown, Md.
List, Leonardo J.....Camden, N. J.

Two applications for membership and one for change of grade of membership are now before the Membership Committee. They have been received since the ballot for new members was issued.

The Tellers of Election of Membership met on November 19 and canvassed the ballots for new members.

The Nominating Committee met on September 29 and selected members to fill the offices falling vacant at this annual meeting.

The Council feels that it has met the wishes of the membership in sending out this year in advance of the annual meeting printed copies of the papers to be read at this meeting.

Your President has appointed the following members as Tellers of Election of Officers: Ex-President John E. Starr, L. Howard Jenks and Theodore Kolischer.

It should be of interest to the membership to know that one of our foreign members, Arthur R. T. Woods, Liverpool, England, has been honored by his fellow engineers in being elected President of the Liverpool Engineering Society, Liverpool, England.

Respectfully submitted,

EDGAR PENNEY, President.

The report of the committee that was appointed at the first annual meeting to conduct tests on the experimental refrigerating plant of the York Manufacturing Company, at York, Pa., was now called for, and ex-President John E. Starr, of the committee, stated that as the York Manufacturing Company were obliged to dismantle the plant before the committee could find it convenient to conduct the tests, the committee had no report to make, and asked to be discharged. President Penney therefore discharged the committee.

The amendments to the By-Laws, introduced at the third annual meeting by W. H. Ross, were now taken up for final discus-

sion and action. After some discussion they were approved. They are as follows:

PROPOSED AMENDMENTS.

"I propose an amendment to paragraph B 24 of the By-Laws for discussion at this, the third annual meeting, and for further discussion and final action at the next, the fourth annual meeting.

"Paragraph B 24 now reads:

"'B24. A Special Nominating Committee, if organized, shall on or before October 20th, present to the Secretary the names of the candidates nominated by it for the elective offices next falling vacant under the Constitution, together with the written consent of each.'

"I would have it read as follows:

"'B24. Twenty or more members entitled to vote may constitute themselves a Special Nominating Committee with the same powers as the Annual Nominating Committee. A Special Nominating Committee, if organized, shall, on or before October 20th, present to the Secretary the names of the candidates nominated by it for the elective offices next falling vacant under the Constitution, together with the written consent of each.'

"When the committee appointed to draft a Constitution and By-Laws made its report, it overlooked the insertion in the By-Laws of a paragraph defining the number of members to constitute and the power of a Special Nominating Committee and I feel that if this amendment is accepted it will cover this oversight.

"I further propose an amendment to the By-Laws by adding a paragraph to be known as paragraph B36, covering the manner of amending the By-Laws:

"'B36. These By-Laws may be amended in the same manner as amendments are made to the Constitution.'"

W. H. Ross, Proposer.

The address of President Penney was now heard. It appears in these Transactions as one of the papers of this meeting.

The session then adjourned for luncheon in the building, arranged for by the Programme Committee.

Second Session, Monday Afternoon, November 80.

The second session of the meeting was called to order at about 2 o'clock P. M. The following three papers were read at this session:

"The Relative Bacteriological Contents of Can, Plate and Natural Ice Under Various Conditions," by John C. Sparks; "Ethyl Chloride Refrigeration," by C. C. Palmer, and "The Construction and Actual Results Obtained from an Ice Making Plant of Moderate Size," by Charles Dickerman.

The following took part in the discussions on the three papers:

On Mr. Sparks' paper—George Berna, Harold W. Cole, Gardner T. Voorhees, the President and Mr. Sparks.

On Mr. Palmer's paper—Albert Johnson, Charles E. Lucke, D.

S. Jacobus, George Berna, R. L. Shipman, George W. Wright, V. R. H. Greene, John E. Starr, the President and Mr. Palmer.

On Mr. Dickerman's paper—Albert Johnson, L. Howard Jenks, Gardner T. Voorhees, D. S. Jacobus, John C. Sparks, A. C. Bishop, Henry Torrance, Jr., Louis Block, Theodore Kolischer, George W. Wright, John E. Starr and Mr. Dickerman.

The report of the Tellers of Election of Officers was now received. It was as follows:

REPORT OF THE TELLERS OF ELECTION OF OFFICERS.

NEW YORK, N. Y., November 30, 1908.

Mr. EDGAR PENNEY, President,
The American Society of Refrigerating Engineers,
No. 154 Nassau Street,
New York, N. Y.

Dear Sir:

Your committee, duly appointed in accordance with the Constitution and By-Laws of The American Society of Refrigerating Engineers to canvass the vote for officers of the Society for the ensuing year, beg to report as follows:

Total ballots received	87
Ballots invalid because not indorsed on outer envelope	2
Of the above ballots—	
Louis Block received for President	84
Karl Vesterdahl received for President	1
Homer McDaniel received for Vice-President	85
Walter C. Reid received for Treasurer	85
Madison Cooper received for Director	82
N. H. Hiller received for Director	82
W. H. Manns received for Director	85
Thomas Shipley received for Director	2
Theodore Kolischer received for Director	1
Peter Neff received for Director	1
John E. Starr received for Director	2

According to the Constitution and By-Laws, the Tellers certify that the following gentlemen are elected:

President—Louis Block.
Vice-President—Homer McDaniel.
Treasurer—Walter C. Reid.
Directors { Madison Cooper. N. H. Hiller. W. H. Manns.

Respectfully submitted,

(Signed) JOHN E. STARR, L. HOWARD JENKS, T. KOLISCHER, } *Tellers of Elections of Officers.*

The President declared the new officers duly elected and appointed ex-President Starr and ex-President Jacobus to escort President-elect Block to a chair beside him.

President-elect Block thanked the members for the honor in the following words:

"Gentlemen, I thank you very much for the honor conferred and the trust and confidence which you have placed in me by electing me President of this Society. I hope I shall deserve this trust and confidence. I want to say to you that I feel just as happy today as Mr. Taft must have felt earlier this month."

The topical discussion, "What Are the Best Temperatures, According to Modern Practice, for Holding Eggs, Butter, Cheese, Meat and Fruits in Cold Storage Warehouses?", was now taken up, and the participants in the discussion were Homer McDaniel, C. H. Parsons, Eugene W. Lewis, Henry Torrance, Jr., L. Howard Jenks, Louis Block, Theodore Kolischer, G. Harold Powell, Irving Warner and Louis Sterne.

The session then adjourned.

Third Session, Monday Evening, November 80.

The third session of the meeting was called to order at about 8 o'clock P. M.

The papers read at this session were: "Waterproofing in Refrigerating Work," by Edward W. DeKnight; "Standard Method of Testing Refrigerating Machines," by D. S. Jacobus, and "Reinforced Concrete Freezing-Tanks," by William M. Torrance.

The following participated in the discussion on the papers read at this session:

On Mr. DeKnight's paper—John E. Starr, John C. Sparks, Theodore Kolischer, D. S. Jacobus, Henry Torrance, Jr., and Mr. DeKnight.

On Mr. Jacobus' paper—Theodore Kolischer, the President, and Mr. Jacobus.

On Mr. Torrance's paper—Junius H. Stone, N. H. Hiller, Thomas Shipley, Irving Warner, Albert Johnson, Henry Torrance, Jr., Theodore Kolischer, the President and Mr. Torrance.

The session then adjourned.

Fourth Session, Tuesday Morning, December 1.

The fourth session of the meeting was called to order at about 10 o'clock A. M.

The following paper was read at this session:

"Performance of Ammonia Compression Machines," by Charles E. Lucke.

A motion prevailed that the discussion on Mr. Lucke's paper

be closed after Thomas Shipley had made some remarks, and that further discussion be presented in writing.

The session now adjourned.

Fifth Session, Tuesday Afternoon, December 1.

The last session of the meeting was called to order at about 2:30 o'clock P. M.

The first business to come before this session was the report on the First International Congress of the Refrigerating Industries. It was presented by J. F. Nickerson and was as follows:

FIRST INTERNATIONAL CONGRESS OF THE REFRIGERATING INDUSTRIES.

The First International Congress of the Refrigerating Industries held at Paris, France, October 5-12, 1908, was, all things considered, a grand success. In point of number of members it exceeded the most sanguine expectations. There were over 6,000 members recorded in forty-one different countries. The number of delegates in actual attendance was over 1,500. There were present government delegates from thirty countries. The United States Government had appointed ten delegates, six of whom were present, viz.: G. Harold Powell, Washington, D. C.; Theodore Kolischer, Philadelphia, Pa.; Gardner T. Voorhees, New York, N. Y.; J. F. Nickerson, Chicago, Ill.; George H. Stoddard, Boston, Mass., and Dr. Mary E. Pennington, Philadelphia, Pa. The number of technical and statistical or general papers presented exceeded 200.

The great interest manifested, not alone by the large attendance and vast amount of voluntary work done on behalf of the Congress, but also by the participation in the work of the Congress by official representatives of the governments, indicates the already large and growing appreciation of the importance of the refrigerating industries in the life and commerce of all nations.

The United States was represented by nineteen delegates, who, with their ladies, made a party of twenty-six. Some seventeen engineering and industrial associations in the United States had appointed delegates to the Congress, not all of whom were in attendance, but some of the delegates present represented two or more organizations of which they were members. At the head of the American delegates was Homer McDaniel, president of the American Committee and the representative of refrigerating and commercial interests of the United States at official functions. He represented the United States with dignity, and in his addresses at the opening and on the closing day spoke with characteristic American directness and brevity.

The United States Government contingent elected G. Harold Powell as their head and spokesman on behalf of the Federal Government delegates, a position which at some of the functions, as at the general opening session, he delegated to Theodore Kolischer. Mr. Kolischer, in fact, was much in demand not only because he spoke fluently in three official languages of the Congress, French, English and German, but because he was always willing, capable and affable. The American delegates were under much obligation to Mr. Kolischer, as by his versatility and constant assistance they were able to accomplish very much more than they otherwise could have done, and thereby their influence in the Congress was greatly enhanced. All the American delegates rendered valuable assistance in the work of the Congress and they were constantly in evidence at the work of the sections.

The American delegation received much attention from the officials of the Congress as well as from the delegations of the various countries represented. Much interest was manifested also in the papers presented by the

American Committee. It seemed to be quite generally recognized that America led in the practical application of the principles of mechanical refrigeration and hence much was expected from the American representatives. It also became evident at the Congress that America led in the scientific and practical investigations into the influence of low temperatures upon perishable produce. This was quite clearly brought out in the scientific and scholarly paper by Dr. Richardson, of Chicago, Ill., and in the excellent papers by G. Harold Powell and Dr. Mary E. Pennington, which were received with most marked attention and deepest interest. The splendid work accomplished in research and investigations, practically applied, evidenced in these papers, will undoubtedly influence Europe and other countries to take up similar work. In fact, resolutions to that effect were proposed and passed.

The effect of the American papers was lessened in many instances by the fact that the authors were not there in person to present them. Where that was done the result was distinctly apparent. An excellent paper on "Comparison of the Production of Cold by Compression of Liquefiable Gases and Other Methods," presented by Gardner T. Voorhees, read by the author and illustrated with lantern slides, was received with marked attention. Of course, all the papers, either from this country or from other countries, could not be read and discussed in the limited time at the disposal of the Congress, and many were passed over, not because they were not considered of equal importance to those discussed, but simply because there was no time.

The proceedings of the Congress and entertainment of delegates were carried out in accordance with the program announced. The problem presented to the management of the Congress by the large attendance of people speaking various languages, and by the vast amount of business offered by the membership from the many countries participating, was a trying one. And the admirable way in which it was carried out, the schedule of the programme, with minor changes only, adhered to and at the time specified, certainly reflect great credit upon the officers of the Congress and especially upon M. Lebon, the president-general, and M. de Loverdo, the secretary-general of the Congress, who assumed the general direction of the meeting and carried out the programme to the letter. M. de Loverdo was untiring in his efforts to look after the comfort and wants of delegates from the many countries, treating all with uniform courtesy.

That some disappointments were experienced might be expected where so vast a quantity of work was attempted with so short a time for its accomplishment. For, although there were nominally five days for work of the sections, the work was cut short on three of the meeting days by special visits to establishments and points of interest in Paris during the afternoons of those days. One of the great lessons of this first meeting, by which subsequent international meetings will profit, is that a limit should be placed to the number of subjects to be considered during a given time.

A number of resolutions were offered and passed in the various sections during the sessions. Many of them referred to purely local conditions, being presented in connection with some paper read or being discussed. Some that were passed were practically repetitions of others passed in some other section. Some were of such minor importance, considered from an international standpoint, that they were omitted from the final number adopted by the Congress as a whole, as told in the proceedings which follow. Some of them were of general importance to the industry at large, but all were interesting as tending to show the trend of thought and feeling in these lines at the Congress.

At the close of the work of the sections a meeting of the English speaking delegates, presided over by M. Lebon, was held, to consider the various resolutions that had been presented in the several sections, by representatives of various nationalities.

Following the above, another meeting was held by the English and German delegates partly to become better acquainted and also to talk over the general work of the Congress and to secure a better comprehension of what was accomplished. Much of the work was lost to them, as they were unable to understand the French in which most of the business of the Congress was conducted.

The meetings were held in the splendid University of Paris, the Sorbonne, which is exceptionally well adapted for such a gathering.

The opening session of the 5th and the closing exercises on the 12th were held in the grand amphitheatre of the Sorbonne, which is a magnificent hall, large enough to accommodate easily the great crowd in attendance. For the actual work of the Congress, which was divided into six sections, there were six separate smaller amphitheatres, five of them opening off or radiating from a central hall or court, one room for each of the first five sections. The sixth section met in a room not far away. It was thus convenient for any one desirous of attending the work being done in different sections to go from one to the other of the halls where the subjects were being discussed, especially as the subjects under discussion were posted upon the respective doors of the halls.

It was right here, however, that no little confusion occurred. In the first place, so many more subjects and papers had been offered than could be reached in the time at disposal in some of the sections that special requests for changes were being continually made to the officers of the Congress. As a result, the order of presentation was altered and it became almost impossible to be on hand when a paper of special interest to an individual was being discussed. However, some of the subjects in the sections overlapped, i. e., a special subject being considered in one section was at the same time an integral part of a paper entered under another title, being considered in another section, and the interested individual in vain wished he could be present at two places at the same time. However, in the main, one could follow the work he might be especially interested in by watching the bulletins posted and going from one section to another, as he could do so readily under the excellent arrangement of halls facing a central court.

The consideration of the subjects treated in the papers offered was very much hindered by the fact that none of the papers, with two exceptions, was as yet in print, only the summaries of the papers having been published. And these summaries were for the most part so inadequate or incomplete that consideration was impracticable, unless the author was at hand to explain, and discussion was of comparatively little value, as no opportunity to study the points at issue was offered.

The greatest benefit of this First International Congress of the Refrigerating Industries will naturally accrue to France, where the actual demonstration of the world's interest in this industry was made, and it will doubtless tend to stimulate favorable government action toward its further development. The great interest aroused will also tend to attract capital in the promotion of companies to apply the benefits of refrigeration. This influence of the international meeting will no doubt also be felt in other countries, though to a lesser extent. The discussion and the reading of literature issued or to be issued by the Congress will tend to reduce prejudice against foods and beverages preserved by means of refrigeration and to reduce official restrictions of importations of alimentary products that can be successfully distributed in the world's markets only by means of refrigeration. In this and many other ways the Congress will no doubt prove of benefit, directly or indirectly, to all countries and to the trade and commerce of the world.

At an International Conference held on Saturday afternoon, October 10, it was decided to form a permanent international organization, and that a permanent international committee should be formed, plans for which are to be submitted by the secretary-general to the various national committees. It was also decided to hold a second international meeting. M. Lebon, who presided, announced an invitation from the Austrian Committee to hold the second International Congress in Vienna in 1910, when an important international exposition is to be held there. It was also announced that the Austrian Government and the city of Vienna had already arranged for liberal appropriations for such a Congress. Mr. Kolischer, on behalf of the American delegates, extended an invitation to hold the second Congress in America. But, at the urgent request of the President and the evident sentiment among the European delegates that there was greater need in Europe of the influence of an International Congress, further with the understanding that the Austrians and French would lend their influence in favor of the

third meeting being held in the United States, the invitation of the American Committee was withdrawn as to the second meeting and Vienna chosen as the next meeting place in 1910.

At a general meeting of the Congress held on Monday, October 12, a number of resolutions which had been passed in the various sections, and which having been somewhat modified and changed, were adopted by the Congress at this meeting. The resolutions that are of particular interest to the refrigerating engineer are as follows:

1.—That an international association be established for encouraging scientific research and other studies of an international character, having its centre at Paris and which, while strengthening the branches of work already specialized, should pursue the study of the entire field of refrigeration.

2.—In view of the great interest attaching to the pursuance and the co-ordination of scientific work in the field of low temperatures, the Congress expresses the wish that the Committee on Section A be charged to proceed on the constitution of a permanent international association for the consideration of all the scientific questions relating to low temperatures.

That an international scientific commission, composed of theoretical and practical refrigerating specialists, should be commissioned to determine the qualities, units and notations applicable to the refrigerating industry to be presented to the next Congress.

1.—That the normal capacity of a refrigerating machine may be determined by the number of thermal units which it can absorb in an hour, for given temperatures of gas in the condenser and in the refrigerator, the selection of said temperatures and thermal units being left to the judgment of the international commission charged with the determination of such units.

2.—That investigation may be made on the properties of superheated vapors of the principal gases employed in refrigerating machines; that to this end, some eminent physicists may be invited to continue the work of Cailletet, of Mathias and of d'Amagnat and extend the study to ammonia and to chloride of methyl, in order to have for these two agents certain data—decisive quantities, specific volumes of the liquid and of its vapor in function of the temperatures and pressures, specific heat of the liquid, etc.

That a study be made, by an international agreement, of improvement of methods of test of refrigerating machines, to be simple, practical and uniform, based on the use of the units determined by the international commission, and applied to the various kinds of machines and to the different circumstances of installation.

1.—That theoretical and professional instruction applied to the various present branches of the refrigerating industry and directed toward new applications, should be instituted in the laboratories and higher technical schools of every country, and completed under the direction of specialists by the practical and detailed study of large refrigerating plants and the rational experimental use of their machines.

2.—That in order to be provided with the necessary scientific and experimental tools, and to be able to meet the expense of experiments, this instruction plan should be aided by the governments, the municipalities, the chambers of commerce, the industrial societies, the agricultural associations and all other societies and individuals who are interested in the refrigerating industry.

3.—That the work of a general nature and the results of the researches of these laboratories and schools, as well as those of the associations of engineers and maufacturers engaged in the refrigerating industry, should be transmitted to the permanent international association and compiled by it, in order that it may be able to publish periodically a bibliographic index, to arrange the reports, and to draw all useful suggestions and conclusions from them, which should be brought to the attention of subsequent international congresses.

In regard to the importance of insulation for the refrigerating industry, the Congress expressed the following views on this subject:

1.—Studies and experiments should be undertaken in the technical schools or laboratories, with known apparatus or that which may later be devised, to determine the specific constants of the various insulators of practical application in the refrigerating industry.

2.—The characteristics and constants to be determined should be the following, taking into account each time the degree of humidity, the density of the material, the conductibility, the flexibility, the resistance to crushing, the moisture absorbing power, the power of absorbing odors, and incombustibility. The constants should be determined under the conditions of temperatures and in thicknesses applicable to the refrigerating industry.

3.—The Congress calls attention especially to the study of conductibility under differences of temperatures, of the thickness, of the degree of humidity and of other causes which might influence this conductibility; for example, the subdivision of the thickness of the same material necessary to secure a given insulation.

4.—The Congress asks that the international committee of the Congress, the formation of which is already decided, constitute an international commission authorized to study the means of testing and compiling the information obtained, with a view to the fixing of uniform methods and of obtaining comparable results, in which researches every latitude is to be allowed to the investigators.

It is desired to place the question of the unifying of said methods as one of the objects of the next Congress, if the researches in question are sufficiently advanced.

To improve the food supply of the people, the Congress resolved that refrigerating installations should be established as soon as possible in the principal centres of production, of consumption and of distribution.

The Congress resolved that the public authorities should favor by every means the installation of cold storage chambers designed for the preservation of food products.

The refrigerating industry having attained a world-wide importance, it is greatly to be desired that scientfic data should be obtained determining the length of time and the temperatures which are best for the preservation of perishable products.

The Congress resolved that the navigation companies, in agreement with their governments, should unite their efforts to furnish Europe with an equipment permitting a more active exchange in perishable products with the colonies.

The Congress also resolved:

1.—That there should be established as soon as possible in all the centres of production of fruits, vegetables, and animals to be slaughtered, refrigerated storehouses permitting the preliminary refrigeration of these products before their shipment by rail to great distances, with or without refrigerator cars.

2.—That the principal maritime ports of commerce should provide cold storage warehouses sufficiently large to accommodate there all the perishable merchandise intended for exportation, or arriving from importations.

3.—That the merchant vessels used for transporting foodstuffs should be provided with reasonable refrigerating installations and varied hoisting apparatus for carrying on an inexpensive but very rapid service.

The Congress further resolved that the plans for the establishment in the principal ports of commerce or warehouses and refrigerated storehouses for perishable foodstuffs, should be undertaken promptly and jointly by the public authorities, the chambers of commerce, the navigation companies and the railway companies.

That the permanent international committee be charged with the collecting of statistics, technical, commercial and financial, which shall serve as a basis for later scientfic and practical work, and call attention to the necessity for practical financial measures, such as advances on merchandise designed to facilitate to producers and merchants the utilization of refrigerated depots.

With a view to extending as soon as possible refrigerating installations in countries which are poorly provided, the Congress expressed the opinion that by way of encouragement these countries should allow refrigerating machines to enter free of duty.

That the international body, the constitution of which is decided, may receive an order to publish, within the shortest time possible, the synoptic

list of the legislative measures and the administrative regulations in force in the different countries, from the point of view of importation and exportation of food products—meat, fish, fruits, etc.—preserved by refrigeration; that instruction in refrigeration may be established in the secondary and higher technical and commercial schools; that the different countries may prepare for the next Congress the bibliography of refrigeration.

The reading and discussion of papers were now taken up. The following papers were read at this session:

"Does Ammonia Disintegrate in Absorption Plants?" by H. Dannenbaum; "Refrigeration Applied to Air Supply for Blast Furnaces," by Bruce Walter, and "The Portable Refrigerating Plant of the United States Department of Agriculture," by S. J. Dennis.

Those that took part in the discussions were:

On Mr. Dannenbaum's paper—Albert Johnson, A. C. Bishop, Gardner T. Voorhees, Henry Torrance, Jr., and Mr. Dannenbaum.

On Mr. Walter's paper—Louis Block, D. S. Jacobus, Gardner T. Voorhees, Thomas Shipley and Mr. Walter.

On Mr. Dennis' paper—Louis Block, Madison Cooper, George Berna, the President and Mr. Dennis

A vote of thanks was extended Mr. Dannenbaum and Mr. Dennis for their valuable papers.

The two topics, "What Are the Theoretical Limitations for the Amount of Ice Produced Per Pound of Coal in Distilled Water Can Plants?" and "What Are the Practical Limitations for the Amount of Ice Produced Per Pound of Coal in Distilled Water Can Plants?" were discussed together at this session, and the following participated in the discussion: Louis Block, Thomas Shipley, Henry Torrance, Jr., R. L. Shipman, D. S. Jacobus, O. J. Morris and Charles Dickerman.

The topic, "When Storing Eggs, Butter, Cheese, Meats and Fruits in Cold Storage Warehouses, Is It Better to Use a Forced Air Circulation or to Pipe the Rooms?" was also discussed at this session. Madison Cooper, Louis Block, Thomas Shipley, John E. Starr, Nelson J. Waite, Gardner T. Voorhees, V. R. H. Greene and Franklin H. Herst took part in the discussion on this topic.

In bringing the business of the meeting to a close, President Penney said:

I wish especially to thank all the members for their tolerance and to say, in excuse for any shortcomings, that this is my maiden effort at presiding over any meeting. I have been particularly gratified at the large attendance, and I certainly have the kindliest feeling toward this Society. I spent quite a number of years in refrigerating and ice making, building refrigerating and ice-making machinery, as you know, but lately I have been drifting out of it. But there is one peculiarly pleasant thing about the meetings of this

Society that it has been my privilege to attend. Both as an officer of the Council and at your open meetings, I have felt perfectly at home, more so than in any other association or society or public relation of my life. Of course, I realize that I am getting to be a back number in refrigeration and I am well satisfied that the work of the Society will go on, ever of increasing value, and that these younger men that come to the front will do better work than their predecessors. And in closing, in fact, I cannot find language to express just exactly how I do feel—but I wish to thank you all for your kind forbearance.

A unanimous and hearty vote of thanks was extended President Penney in appreciation of his efforts on behalf of the Society.

The meeting then adjourned.

MEMBERS AND GUESTS ATTENDING MEETING.

Austin, A. O., New Rochelle, N. Y.
Bailey, P. H., New York, N. Y.
Barker, H., New York, N. Y.
Bates, Guy, Whippany, N. J.
Bender, Adolf, New York, N. Y.
Bernhard, Conrad, New York, N. Y.
Berna, George, Ithaca, N. Y.
Birkholz, Julius W., New York, N. Y.
Bishop, A. C., Detroit, Mich.
Blechner, Emil F., New York, N. Y.
Block, Louis, New York, N. Y.
Bonner, Richard O., New York, N. Y.
Borgstedt, H., New York, N. Y.
Borchardt, Albert H., New York, N. Y.
Bower, William H., Philadelphia, Pa.
Bowers, James, Wilmington, Del.
Briggs, John N., Coeymans, N. Y.
Brownell, C. H.,
Washington Court House, Ohio.
Burhorn, Edwin, New York, N. Y.
Campbell, W. H., New York, N. Y.
Chatard, William M., Baltimore, Md.
Coffey, B. H., New York, N. Y.
Cole, Harold W., New York, N. Y.
Cooper, Madison, Watertown, N. Y.
Day, Abram, Bloomfield, N. J.
Dannenbaum, H., Philadelphia, Pa.
DeKnight, Edward W.,
New York, N. Y.
Dennis, S. J., Washington, D. C.
Dettmar, E., New York, N. Y.
Dickerman, Charles, Philadelphia, Pa.
Dixon, H. M., New York, N. Y.
Ebling, Louis M., New York, N. Y.
Edwards, Harry M., New York, N. Y.
Ely, James, New York, N. Y.
Fairbanks, F. L., Boston, Mass.
Farish, J. B., New York, N. Y.
Feld, Clayton O., York, Pa.
Fisher, H. R., Philadelphia, Pa.
Fitzsimmons, J. E., New York, N. Y.
Frantz, S. R., Waynesboro, Pa.
Friedmann, E. N., New York, N. Y.
Goerner, G. W., New York, N. Y.
Greene, V. R. H., New York, N. Y.
Guckel, Charles H., Colon, Panama.
Gueth, Oswald, New York, N. Y.
Guldin, C. Q., Reading, Pa.
Harden, G. M., Philadelphia, Pa.
Haire, D. E., Philadelphia, Pa.
Hargreaves, William,
New York, N. Y.
Havenstrite, Charles D.,
Newark, N. J.
Herst, Franklin H., Philadelphia, Pa.
Heyrodt, H. F., Bridgeport, Conn.
Hexamer, W. E., Philadelphia, Pa.
Hill, George F., New York, N. Y.
Hill, Walter L., Boston, Mass.
Hiller, N. H., Carbondale, Pa.
Hodges, H. F., Philadelphia, Pa.
Holden, D. L., New York, N. Y.
Horne, Frank A., New York, N. Y.
Hough, C. A., New York, N. Y.
Howe, Arthur E., New York, N. Y.
Jamison, J. V., Jr., Hagerstown, Md.
Jacobus, D. S., New York, N. Y.
Jenks, L. Howard, New York, N. Y.
Johnson, Albert, St. Louis, Mo.
Judd, F. F., Boston, Mass.
Kehoe, Robert P., New York, N. Y.
Kershaw, G. E., New York, N. Y.
Keller, P. P., New York, N. Y.
Kelly, Arthur W., Red Bank, N. J.
Kolischer, T., Philadelphia. Pa.
Laiman, Theodore W.,
Hartford, Conn.
Langdon, George M.,
New York, N. Y.
Lewis, Eugene W. Cincinnati, Ohio.
Livezey, John R., Philadelphia, Pa.
Lowe, Allen, New York, N. Y.
Lucke, Charles E., New York, N. Y.

MEMBERS AND GUESTS ATTENDING MEETING—Continued.

Manns, W. H., Waynesboro, Pa.
Mallon, M. F., New York, N. Y.
Markel, E. J., New Haven, Conn.
Mardon, Arthur, New York, N. Y.
Marshall, A. T., Hartford, Conn.
Matthews, F. E., New York, N. Y.
Mayer, Fred W., Jersey City, N. J.
McCarthy, George L., New York, N. Y.
McDaniel, Homer, Cleveland, Ohio.
McDoal, Grant, New York, N. Y.
Miller, Joseph B., Philadelphia, Pa.
Minck, Peter, Jersey City, N. J.
Morrison, H. H., New York, N. Y.
Morse, Louis S., York, Pa.
Neubecker, Jean, New York, N. Y.
Neubert, M. E., New York, N. Y.
Newham, H. S., New York, N. Y.
Nolde, Fred, New York, N. Y.
O'Brien, Douglas J., New York, N. Y.
Oliver, Albert, New York, N. Y.
Owens, M. F., Hartford, Conn.
Palmer, C. C., New York, N. Y.
Palmer, G. W., New York, N. Y.
Parsons, Charles H., Springfield, Mass.
Penney, Edgar, Newburgh, N. Y.
Phillips, A. M., New York, N. Y.
Phillips, Louis A., New York, N. Y.
Pilsbry, F. W., Chicago, Ill.
Powell, Harold G., Washington, D. C.
Ridgeway, F. B., Poughkeepsie, N. Y.
Riley, W. H., Wilmington, Del.
Robinson, F. G., York, Pa.
Roder, W., New York, N. Y.
Rooksby, Edwin J., Philadelphia, Pa.
Ross, W. H., New York, N. Y
Sargent, Allston, New York, N. Y.
Severn, William B., Philadelphia, Pa.
Shipley, S. J., New York, N. Y.
Shipley, Thomas, York, Pa.
Shipley W. S., New York, N. Y.
Shipman, R. L., Ithaca, N. Y.
Smith, Guy B., Brooklyn, N. Y.
Smith, Edwin R., Port Washington, Wis.
Soule, T. H., New York, N. Y.
Sparks, John C., New York, N. Y.
Starr, John E., New York, N. Y.
Sterne, L., London, England.
Stevenson, S. P., Chester, Pa.
Stone, Junius H., New York, N. Y.
Stow, Charles F., Philadelphia, Pa.
Stratton, Harrison D., Philadelphia, Pa.
Strickler, A. B., York, Pa.
Stutsman, J. A., Boston, Mass.
Sutton, W. S., New York, N. Y.
Tinker, H W., Boston, Mass.
Torrance, Henry, Jr., New York, N. Y.
Torrance, William M., New York, N. Y.
Van Buren, Wilson, New York, N. Y
Vesterdahl, Karl, New York, N. Y.
Voorhees, Gardner T., Boston, Mass.
Waite, N. J., Cleveland, Ohio.
Walter, Bruce, Pittsburg, Pa.
Warner, Irving, Wilmington, Del.
Webber, W. J., Boston, Mass.
Wegemann, Karl, New York, N. Y.
Weintraub, Wistar M., Philadelphia, Pa.
Werliin, Godwin, Philadelphia, Pa.
Werliin, L., Philadelphia, Pa.
Wegner, G. A., Canandaigua, N. Y.
Williams, E. T., New York, N. Y.
Wills, James, New York, N. Y.
Winborg, A., York, Pa.
Woodcock, Willard J., New York, N. Y.
Woolson, C. G., Newark, N. J.
Wright, F. E., Cleveland, Ohio.
Wright, George W., Baltimore, Md.
Yeakley, L. F., Philadelphia, Pa.

THE BANQUET.

The banquet was held at 8 o'clock on the evening of the second day of the meeting, December 1, at the Hotel Victoria, New York, N. Y. An excellent menu was served, and about one hundred members and guests enjoyed the occasion.

Ex-President John E. Starr acted as toastmaster in his usual delightful and entertaining manner.

The speakers of the evening were President Louis Block, Vice President Karl Vesterdahl, Theodore Kolischer, Philadelphia, Pa.; Gardner T. Voorhees, New York, N. Y.; Louis Sterne, London, England; Junius H. Stone, New York, N. Y.; Thomas Shipley, York, Pa.; William H. Ross, New York, N. Y.; O. J. Morris, Dallas, Texas, and Henry Torrance, Jr., New York, N. Y.

The benediction was made by Franklin H. Herst, Philadelphia, Pa.

Among those attending the banquet were the following:

Bates, Guy, Whippany, N. J.
Bishop, A. C., Detroit, Mich.
Blechner, Emil F., New York, N. Y.
Block, Louis, New York, N. Y.
Bower, William H., Philadelphia, Pa.
Borgstedt, H., New York, N. Y.
Briggs, J. N., Coeymans, N. Y.
Burhorn, Edwin, New York, N. Y.
Campbell, W. H., New York, N. Y.
Chatard, William M., Baltimore, Md.
Coffey, B. H., New York, N. Y.
Cole, Harold W., New York, N. Y.
Dannenbaum, H., Philadelphia, Pa.
Dennis, S. J., Washington, D. C.
Dettmar, E., New York, N. Y.
Dickerman, Charles, Philadelphia, Pa.
Dixon, H. M., New York, N. Y.
Ebling, Louis M., New York, N. Y.
Edwards, Harry M., New York, N. Y.
Feld, Clayton O., York, Pa.
Fisher, H. R., Philadelphia, Pa.
Fitzsimmoons, J. E., New York, N. Y.
Goerner, G. W., New York, N. Y.
Guckel, Charles H., Colon, Panama.
Hargreaves, W., New York, N. Y.
Herst, Franklin H., Philadelphia, Pa.
Hexamer, William E., Philadelphia, Pa.
Hill, George F., New York, N. Y.
Hill, Walter L., Boston, Mass.
Hiller, N. H., Carbondale, Pa.
Howe, Arthur E., New York, N. Y.
Jacobus, D. S., New York, N. Y.
Johnson, Albert, St. Louis, Mo.
Kehoe, Robert P., New York, N. Y.
Keller, P. P., New York, N. Y.
Kolischer, T., Philadelphia, Pa.
Laiman, Theodore W., Hartford, Conn.
Langdon, George M., New York, N. Y.
Livezey, John R., Philadelphia, Pa.
McCarthy, George L., New York, N. Y.
McDoal, Grant, New York, N. Y.
Manns, W. H., Waynesboro, Pa.
Mardon, Arthur, New York, N. Y.
Markel, E. J., New Haven, Conn.
Marshall, A. T., Hartford, Conn.
Mayer, Fred W., Jersey City, N. J.
Miller, Joseph B., Philadelphia, Pa.
Morse, Louis S., York, Pa.
Neubecker, Jean, New York, N. Y.
Neubert, M. E., New York, N. Y.
Newham, H. S., New York, N. Y.
Nolde, Fred, New York, N. Y.
Penney, Edgar, Newburgh, N. Y.
Phillips, A. M., New York, N. Y.
Roder, W., New York, N. Y.
Ross, W. H., New York, N. Y.
Sargent, Allston, New York, N. Y.
Shipley, S. J., New York, N. Y.
Shipley, Thomas, York, Pa.
Shipley, W. S., New York, N. Y.
Smith, Guy B., Brooklyn, N. Y.
Soule, T. H., New York, N. Y.
Sparks, John C., New York, N. Y.
Starr, John E., New York, N. Y.
Sterne, L., London, England.
Stevenson, S. P., Chester, Pa.

MEMBERS AND GUESTS ATTENDING BANQUET—Continued.

Stone, Junius H., New York, N. Y.
Stratton, Harrison D., Philadelphia, Pa.
Strickler, A. B., York, Pa.
Stutsman, J. A., Boston, Mass.
Sutton, W. S., New York, N. Y.
Torrance, Henry, Jr., New York, N. Y.
Vesterdahl, Karl, New York, N. Y.
Voorhees, Gardner T., Boston, Mass.
Waite, N. J., Cleveland, Ohio.
Walter, Bruce, Pittsburg, Pa.
Webber, W. J., Boston, Mass.
Wegemann, Karl, New York, N. Y.
Weintraub, Wistar M., Philadelphia, Pa.
Werliin, L., Philadelphia, Pa.
Werliin, Godwin, Philadelphia, Pa.
Winborg, A., York, Pa.
Woodcock, Willard J., Brooklyn, N. Y.
Wright, F. E., Cleveland, Ohio.
Wright, George W., Baltimore, Md.
Yeakley, L. F., Philadelphia, Pa.

PAPERS

OF THE

FOURTH ANNUAL MEETING

OF

THE AMERICAN SOCIETY OF REFRIGERATING ENGINEERS

No. 50.

PRESIDENTIAL ADDRESS.

By Edgar Penney, Newburgh, N. Y.

The engineer's knowledge of his art is mostly derived from experience or experiments; the teaching of both has been passed from man to man, handed down from generation to generation, finally to be recorded in books. The data from experiments collected by different men under varying conditions, if carefully kept and published, are thus made available for analysis by some painstaking, scientific person, and may ultimately lead to the discovery of an underlying principle or recognition of natural law governing the particular phase of matter or thing experimented upon.

Of experiments there have been many. Man, of an inquiring mind, experimented at the beginning, and will continue his experiments to the end. Without the knowledge thus gained he would today be as helpless as the brute animal left to its own resources. We feel secure only when following in the footsteps of others along beaten paths. We may pride ourselves upon our fund of knowledge, but it is the unsolved things that bring to us a realization of our ignorance; grappling with new problems, we feel our weakness; our bridges, as it were, burned behind us; at sea, rudder gone, without chart or compass; our established theories, our scientific knowledge of other things, lend little aid in such a dilemma other than that of logical methods of attack. Theory then seems mere speculation, and the sciences we know not correlated to the subject matter sought. Gropingly, we are led to inquire the meaning of the words theory and science. The first is defined as supposing, or pure and simple guessing. In a scientific sense, theory may mean an explanation that does not really explain but appeals to the senses with such force that it seems logically true; or it is a hypothesis supported by a certain amount of commonsense evidence.

Science, broadly speaking, is to know; knowledge severely tested, especially when it refers to the laws of nature. We have, however, many sciences, such as abstract science, relating to logic and mathematics; abstract concrete science, which includes me-

chanics, physics, chemistry, etc.; concrete sciences, treating of geology, biology, astronomy, sociology, etc. Exact science, so called, is a thing that can be demonstrated to be true. Truth is the word, and to discover and demonstrate truth is worth all it costs in time, money and even life itself. Deductive science is to reason from cause to effect or vice versa; inductive science rests on evidence more or less convincing, but, while not really science, its use may lead nearer and nearer to the truth; as, for example, experiments repeated, based on scientific methods. Science is knowledge reduced to law, embodied in a system.

Theory, then, is speculation, and science is limited to known or accepted facts; things we may have been taught to believe as truths; but as the world progresses investigation and experimental knowledge may change our science by the discovery of new facts, establishing new truths, upsetting old schools and formulas.

Man is so constituted mentally that he keeps guessing, keeps everlastingly digging away for facts; satisfied only when he finds out the why and wherefore, and having found it seeks fresh problems to solve, and thus our knowledge grows.

One truth discovered, one fact determined, is a stepping stone to another. We measure, or, at least, we hope we do, nearer and nearer the ultimate possibilities of scientific truth, but we know not as yet the real limitations, the last analysis of science; nor do we always realize what prevents a further and more rapid advance, but we can guess what is standing in the way of greater achievements; nothing, simply nothing, except our ignorance of the varied operations of nature's laws, how best to complete discoveries.

Experimental knowledge unfolds the secret of the fundamentals all too slowly; while our growing knowledge is accumulative it is not always accelerative, as there are still too many gaps or missing links. It has been slowly gathering since the dawn of science. Some of the links were known to a few of each generation; they must all be known to the many to find general application in the useful practical arts.

What is art? Skill in planning and executing. Practical application of the teachings of science. In a mechanical sense, those arts which can be successfully applied by men who possess only the genius to work with the trained hand and eye, but lacking imaginative, creative minds. Art and science are closely related. Art is something to be done; science, something to be known; science is urged on by art; art is perfected science. Theory! Science! Art! Words spoken every day, and many times with a show of superior wisdom.

It is given to an occasional individual to realize the full significance of these terms. The world has produced a limited number who have been blessed with gifts to discover and harness certain forces of nature, thus placing in the hands of their fellow men another means to overcome life's problems.

Would that we were all great inventors, great discoverers! Perhaps Divine Providence or the inherent laziness of some of us may be at fault. I cannot say, except for myself. The other fellow got the spark of genius intended for me—and then, again, we are quite apt in cultivating the line of least resistance because it is easier to work with hand than with head; less mental effort to be an artisan than a scientist.

Therefore, we have the theorist, the scientist, the genius, the humble practitioner of art; besides, we need not look far for the man who proclaims that he knows what he knows, but we think he only thinks what he thinks.

We hear much of up-to-date systems; things connected according to a scheme by a schemer; systems galore, scientific, artistic and realistic, cure-alls, but some of them misfits, depending upon the human factor to operate them to make them square with results worth having. Being unscientific, they are quickly sidetracked and forgotten.

There are men who have no patented systems; no scientific method that will bear analysis; lonely geniuses who are certainly making a success of life. Possessed of a strange intuition, unspeakable, appearing to follow no law, written or unwritten, their bridges stand; their boats beat world records; machinery, product of their strangely well-equipped mentality, excels. That they were able to give us the secrets of their success! They seem the doers, the thinkers, not the talkers with an easy flow of language.

Science sways men; in some cases, however, it is simply a case of atmosphere, a "state of mind," a gentle commotion of the brain. We do it because the other fellow did it, and he won out, forgetting that it may not fit our case; but there is more than one way of doing a thing. Sometimes, in my perplexity, I find there are too many ways of doing a thing, and I always wish I knew the right way. There are some who claim there is only one way—that is their way.

We, however, sit up and take notice when some unheralded genius from the backwoods shows a new trick and sets a faster pace. We know a good thing when we see it and trot along in his procession, unless we are blessed with sufficient creative originality to swing clear of the crowd.

In order to advance in his procession the engineer must be receptive, unprejudiced, a tireless student—a searcher for the truth, for the sake of truth. To many this may seem impossible, purely academic advice, if our daily bread depends upon our practicing only those precedents that bring commercial success and insure steady employment; especially those conservative ones who are operating industrial establishments, appearing to have no time for ideals, much less for putting them in practice, especially when it involves the thankless task of educating the dear buying public to the higher plane, this manufacturer generally having to pay all tuition bills, including the customer's board and lodging. Under the circumstances we are perfectly willing that the experimenting pioneer, the advance guard of science, shall bear the brunt and cost, but we try to keep within speaking distance of the fellow who goes ahead and does the work. Sometimes we call them cranks.

Good engineering practice is a term in common use in our profession. What is good practice? More important still, what is practice? The last is easy if you secure the best known economy, all things considered, for the lowest price; if so, you have the best.

Good practice, however, may be mediocre; a rather low level of attainment, but, nevertheless, safe to follow when you are in doubt and cannot learn better. Keep yourself posted on the highest state of the art in which you are engaged. Start, if possible, where successful men leave off. Most of them have private scrap heaps. It keeps their conceit within bounds to recall occasionally blasted realizations of their boundless enthusiasm, and ambitions lie in cold storage, as it were. If privileged, look their exhibits over; it may inspire you to do better than the owners of the junk piles; or, at least, not to attempt to thresh the problems all over again—just because you think you are so brilliantly smart you can succeed where they have failed. To my mind these scrap heaps teach the necessity of scientific method and proper preparation for one's work. Time spent in necessary calculations and examining recorded data of others' successful work, completing and studying your own plans on paper, is inexpensive, and you should know whether what you want to do is worth doing at all. Much cheaper this than hastily starting a scrap heap all your own—for example, but one in a thousand patents has any value.

If you can continue in business burdened with a scrap heap, as some of the others have done, you will agree that knowledge gained by the junk route is expensive, and it will finally dawn on you that the last thing you did should have been done first. It has been my

fortune to go through several successful concerns' scrap heaps; I found the same discarded and impossible things in each, the final evolution singularly leading to similar procedures, methods of design and manufacture arrived at independently each of the other. It occurred to me that as the scrap heaps are identical, and the results similar, why not begin where they left off?

Spread the knowledge of success and the reason for each success. Let the failures be known and why they failed; history may repeat itself. There is nothing really new, especially in scrap heaps.

Engineering mistakes are costly. It is not always the ignorant, unscientific engineer who makes the big fizzles. He does not often get the chance. It is the careless, often scientifically trained and educated one who, by reason of his reputation and supposed ability, is entrusted with expensive construction, and thus has the greater opportunity to make mistakes. Because of his success in the past he may have bred a contempt for tedious scientific methods; in his conceit he may imagine he cannot make a mistake; that he has cultivated by constant practice of his art an instinctive sense of comparative proportion, automatic in its application to any problem with which he has to deal. Routine familiarity with his specialty, he flatters himself, has put him in training so he can settle any problem offhand with his eyes shut, and so he risks it by following some of his own misfit precedents.

Another class who make mistakes are those who try to figure but cannot. They figure at a thing and they figure all around it without hitting the bull's-eye; they have the idea from the book, but not the mathematical training or inspiration born of experience.

There are other engineers who cannot comprehend from drawings they make any sense of proportion or just how the finished machine or its detail parts will look when done. They do not recognize their own completed machine in the shop when they see it, it looks so strangely different, and would like to disown many parts of it if they could. These are the men who make wooden models to show themselves what the thing looks like.

You will agree that the above examples should be catalogued as unsafe engineering. Do not do things differently, however, from the other fellow if his is the better way and you are free to use it.

We are in debt to our predecessors because of their contributions to our arts and sciences, which enable us, if we will, to start where they left off; if we study their work we need not start as they began.

Young men of the present have this great advantage; stand firm

on the advanced ground; leave something more than a failure behind for the use of the next generation.

Success may come through many failures, but avoid the failures if you can. It is a costly way to educate one's self. If your boss has to pay for your failure you will certainly lose your job. If you think you can work a revolution in the mechanical arts by masquerading an old principle or application in a new dress, sleep over it. It may seem otherwise in the morning.

If enthusiastic or impartial you will, of course, to satisfy yourself have to build it in brass, iron, wood or steel, but it may look to you so different when erected, and act so contrary when you try to run it, that you finally admit you forgot something, and that important something sends it to the junk dealer.

A farmer drove a pig from his corn field. He examined the fences and found them tight, but the pig got back. The puzzled farmer, watching, saw the pig enter a hollow log lying through the fence. The pig was again driven out, and the farmer removed the log, but the pig made straight for the log, and, following precedent, went in at the same end it had always used. It crawled through, and, astonished but not disheartened, found itself outside the corn field. Grunting, it repeated the experiment again and again, always going into the same end of the log, coming out at the other end, and more surprised each trip at its failure to land in the corn field. Moral—Notice where the hole leads before you crawl in. If you make one trip do not repeat it.

A Scotch manufacturer, with an established reputation for making black goods, guaranteed always fast color, proudly invited a chemist friend to see his mill. On reaching the weaving room the looms were making such a noise that they could not talk. When outside, the chemist asked, "What's the matter with your looms? They make a fearful noise." The weaver whispered, "That's a secret, but I do not mind telling you I have found by experience and experiment that the noisier my looms the faster are my colors." "Give me a piece of the undyed cloth," said the chemist. Examination showed him that the wear and abrasion of the metal parts of the looms had liberally sprinkled the cloth surface with iron dust, which went with the cloth to the dye tubs, and therein was the secret of the fast color. The chemist, for a consideration, of course, gave the manufacturer a solution to mix with his dye and thus fix his color—certainty of results without noise. This was a case of scientific method versus untutored experience slightly tinged with superstition.

George H. Corliss, the inventor of the Corliss engine, possessed

in a high degree the scientific, analytical mind. In speaking of his engine, which, by the way, was re-designed as higher steam pressures and higher speed were introduced, he remarked: "I have calculated my engine so that one part is as strong as another, and in an experience of many years I find some of the engines have broken down, but never the same part twice. One engine may break a crank, one a connecting rod, another this part, another that, so I conclude my engine is like the deacon's "one-hoss" shay—when it breaks it is apt to break all over. This is an example of combined scientific and experimental knowledge.

The lawyer swears by precedents; he pins his faith on winning if he finds them. The engineer makes his own precedents as he goes along by scientific methods applied in his more important work, where to use old proportions might invite disaster. He must be satisfied by investigation and analysis that his plan is safe, and the best plan, all things considered. We recognize the value of precedent applied more particularly to the commercial branches of engineering, routine manufacturing, when building so-called standard types of machinery—here conservatism; the influence of competition; the desire to leave what is regarded good enough alone; the expense of introducing a new thing, especially when the purchaser questions the value to him personally, and kicks at the price he has to pay for the better thing; conditions discouraging, sometimes seemingly prohibitory, but often worth while to surmount.

Some engineers seem to doubt their own ability to improve upon what has been done so well before, and rather than run risks and loss of profit by introducing features more or less experimental, adopt with all their faults constructions which have the prestige of a noted name and may stand in the market as peculiarly representative. Errors are thus perpetuated. Faults may even be extolled as virtues of necessity or evidences of the original designer's genius introduced for some reason all his own, too profound for imitators to change.

Individual whims or hobbies of some designers are much in evidence. So much so that it is often quite possible, by inspection of a machine, to say who designed it. I believe in impressing your individuality on your work, but too often the unreasonable introduction of several hobbies gives the designer an unenviable reputation.

By a strange process of reasoning features of extraordinary proportions are employed in certain machines, the functions of which are closely related to those in common use on other prime motors. The designer when asked why he does not adopt the standard construc-

tion will reply: "Why, this is for an ice machine. You've got to have it that way because it is an ice machine," and the man who builds a brickyard engine explains that because it is to make brick it must be special, and so on up and down the line. You may call it traditional or irresponsible engineering, if you choose. The gas engine man strikes out for himself, the pump builder, the stationary engine builder, all ignoring the law of scientific proportions applicable to all without recourse to peculiar shapes.

In my mail not long ago I received specifications from three engineering firms—one in Boston, one in New York, one in Philadelphia; singularly enough they were all for cross-compound condensing engines, direct-connected generators, specified the same steam pressure, same horse power, same speed, all different installations not related to each other. Number 1 indicated a crank shaft 12 inches in diameter, bearing 24 inches long; Number 2's shaft must be 14 inches in diameter, bearing 26 inches long; Number 3 asked for 16 inches diameter, bearing 30 inches long. The engines were intended, respectively, for electric railway work; electric light station; the last, with the big shaft, for a manufacturing concern. As the specified duty was similar I lost faith in consulting engineers for about a quarter of an hour, and then I gave each a bid on what he asked without accepting responsibility, and as the engines throughout were proportional to shaft diameter, each got a price on a different engine to suit his peculiar ideals; no two built from the same patterns. Here was a chance to do a little missionary work. I think it would be a good thing for consulting engineers to compare notes, and sometimes ask the experienced engine manufacturers who have handled thousands of such cases what they know about engines. Refrigerating engineers might also inquire of the busy ice-machine builder what he had found out about his business.

I wished to buy a four head, 10x10 foot planing machine, 20 foot bed. In response to my inquiry I received figures about as follows:

A.....	$20,000;	weight, 240,000 pounds.
B.....	15,000;	weight, 180,000 "
C.....	12,500;	weight, 120,000 "
D.....	10,000;	weight, 90,000 "

The last machine was an 8-foot planer widened to 10 feet.

The 240,000-pound machine was good for slugging oil-tempered armor plate. The 180,000-pound machine was for planing cast steel. The 120,000-pound machine was just for ordinary everyday use in a general machine shop. Not very bad examples, these, as you should

buy the machine adapted to your work. I wound up by purchasing a second-hand planer, as the only work I had for it was large castings, very light and simple facing of joints.

Precedent, however, like tradition, exerts upon us all, consciously or unconsciously, an influence not to be ignored, and governs our engineering practice to such an extent that we follow more or less without questioning. The mere following of established precedents without trying to understand the reason why is the prerogative of a lazy or incompetent engineer. But bear in mind that modern conditions are changing; science advances so rapidly that precedent becomes more and more unsafe and a matter of ancient history to be ignored by the engineer of today, who must use scientific methods and proportions based on experimental scientific knowledge and accurate tests of materials employed.

Precedent, as practically applied, means that if an engineer builds a bridge, and it stands, it is thus experimentally proven to be safe. He will, therefore, build a bigger one along the same lines. If it does not break down we may stretch our theory of construction until our latest bridge fails, and then, too late, we find the folly of precedent without discrimination to avoid unnecessary risks. Precedent or copying is, of course, much easier than original research, the tedious finding by use of mathematics and tests the proper proportions.

I am not quite in sympathy, however, with scientific hair splitting often applied to the ordinary problems of engineering, but I do believe it is necessary to be personally able to calculate all the important features, and to know the maximum per cent. of error involved by the use of any labor-saving approximation or short cut formulas that we may use. It is better to figure to one place of decimals intelligently than to calculate to seven places, and then by reason of inexperience remain in doubt.

To be deemed scientific some engineers theorize wisely, figure endlessly, and in the multitude of refinements thus created in their confused minds lose sight of the prime function of the machine and correlation of its parts, constantly narrowing their mental vision that should be trained to take in the general broad perspective, comprehensively grasping the whole.

Take care of the essentials and the details naturally fall in line or are easily adjusted. Block out and perfect the general construction, thus fixing in your mind, first in its entirety, then minor details. This distinguishes the scientific methods of a designer compared with those of a simple draftsman; a mechanic compared with a workman.

Do not cajole yourself, however, into the belief that after much thought and burning of midnight oil everything you have settled is strictly scientific. You may have an elegant theory for this detail, a mathematical demonstration for that feature, a profound reason for the other, and the whole, you argue, is perfection ready to be realized; perhaps, upon experimental trial you may be discouraged and, your purse emptied by failure, forced to take employment with some successful engineer who has learned how not to deceive himself by those infinitesimal fractions, but whose product of brain demonstrates the possession of a searching, creative, adaptive mind. Fortunate are you if you can without prejudice, with receptive mind and eye, secure for a time the privilege of association with such a man, and be able to make the most of your opportunity.

Stir a man's ambition to excel. Fill him with a consuming desire to do something, but help him to learn how to do it, teach him how to do it right. He may start in a most commonplace manner, but, the spirit awakened, he will keep on climbing, learning more, and learning more on scientific methods as he advances. To such, to all engineers, work on, work ever, for the glory of work well done. The times we live in demand more power, higher thinking, better results, reduction in first and final cost, and, most important, the conservation of our natural resources by avoiding needless waste. The engineer is the man to whom of all men best equipped this demand comes. He must meet it by measuring up to the full specification or requirements, and take advantage of the opportunities presented.

The American Society of Refrigerating Engineers has for its purpose to help each member along engineering lines. We do not meet as manufacturers or personally to grind our individual axes, but as scientific engineers. Let each contribute his share. Whether he be a scientist, genius, or an artist practicing the art, a good talker without thinking or a bad talker but a good thinker, he has a place here. All are asked to contribute, and by so doing each reaps the greatest benefit from association with others of kindred ideals.

When we speak of a refrigerating engineer eligible to membership in this Society, our constitution specifies that he must be competent to design or construct, to take responsible charge of work in his branch of refrigerating engineering, and we are led to inquire how many branches are there in refrigerating engineering. What are they? If we represent branches only, where and what is the main stem? We admit without question, however, that refrigerat-

ing engineering is a branch of mechanical engineering, the main trunk, and that refrigerating engineering may be one of the main boughs which again divide into minor branches. This narrowing view of refrigerating engineering opens the door to any aspirant for membership who has anything directly to do with a twig or offshoot of a branch of the main bough springing from the main trunk of the tree, mechanical engineering; and here again we are up against the claim that mechanical engineering is simply a branch of civil engineering. But to return to our own twiglets of refrigerating engineering, new applications of mechanical refrigeration are constantly being made. They open the way to specialization, but as a general rule all the unusual applications thus far made call for no departures in form or substance of existing practice broadly considered as constructive and applied refrigerating engineering.

It is, of course, necessary in some cases to determine by trial the best arrangement to meet the new applications as they appear, but this is a function of the all-around experienced and scientifically trained refrigerating engineer. To be successful you must be posted in broad principles, experienced in general practice before you can specialize. A refrigerating engineer should be competent in all the branches of his art. The upward trend of the Society should be to help its members to become representative refrigerating engineers in its broadest sense; the mere fact that you are a member should give assurance to your clients of high standing in the profession of refrigerating engineering, competent in any and all its branches.

The profession of refrigerating engineering has been further dignified and assumed greater responsibilities, its growing importance finding international recognition by the calling of the first Congress of the Refrigerating Industries recently held in Paris. Here were gathered men of science, engineers, managers of industries, distinguished delegates from all countries, many of whom held high official positions, and were especially assigned by their respective governments to co-operate and assist by giving and receiving scientific information in relation to the practice and theory of refrigeration in all its branches. The congress has done much, and paved the way for others to be held in the future when the science and theory of our art will be more firmly established along scientific lines.

As our Society was represented by delegates, we hope, if our long programme permits. to hear from them.

Permit me in closing to call attention to the unusual excellence of the papers to be presented at our professional sessions, and to

suggest, owing to the length of our programme, that all discussion be well considered and to the point, strictly following the subject matter of the paper.

I trust and request that all personalities and irrelevant remarks be omitted. Please do not undertsand me to mean that personal opinion should not find expression—only the manner of presentation.

This is a meeting of engineers for the purpose of personal interchange of knowledge pertaining to our profession, and should be conducted in harmony and with dignity.

No. 51.

THE RELATIVE BACTERIOLOGICAL CONTENTS OF CAN, PLATE AND NATURAL ICE UNDER VARIOUS CONDITIONS.

By John C. Sparks, New York, N. Y.

(*Associate Member of the Society.*)

In the past few years a great deal of attention has been directed to the presence of bacteria in ice. Under normal conditions, can, plate and natural ice is free from pathogenic organisms, that is, organisms capable of passing disease on; but under favorable conditions these three types of ice have been found to contain large quantities of these bacteria, and as such conditions are due to lack of care and are easily preventable, a study of the subject is worth while.

It is an undisputed fact that, with the proper precautions, the distilled water used for making can ice is sterile and free from any organisms whatever, yet if due care is not taken the distilled water may be worse, from a sanitary and bacteriological point, than the raw water used in the boilers.

The first case I will take up relates to a can ice plant in a certain city, which was examined by the local board of health. It was found that the ice contained nearly ten times the quantity of bacteria that was in the raw water before distillation. I do not consider it fair to publish in this paper the names of the plants I am taking as "horrid examples," but I have all the original documents from the various boards of health to show any "doubting Thomas." Every plant, when shown the conditions which existed, immediately took steps to remedy the trouble, so contrition removed all offense. Then, again, as I will show later, the fault was in a large measure due to the design of each plant, so that the operators were not alone to blame.

To revert to the case of this can ice plant. The pathologists of the city in question wrote the following letter to the president of the board of health:

"Dear Sir—We have been making of late a series of examina-

tions of the artificial ice now made and sold in this city, and especially of that made and sold by the so-called Hygeia Ice Company.

"This ice, it is claimed, is made from pure distilled city water, and under that pretense is sold.

"Our analyses of this ice show that it often contains large numbers of living bacteria, on one occasion as many as 1,134 in one cubic centimetre of the melted ice.

"Now, it has been determined that a considerable proportion, though not all, of the bacteria in water may be killed by the freezing process; as a rule from 80 to 90 per cent. are thus destroyed.

"Analyses of the city water, made at the same time that the ice was examined, showed that it contained on the average about 150 bacteria to one cubic centimetre.

"It follows from the fact that over 1,100 germs may be left alive in one cubic centimetre of the melted ice of this company that the water from which their ice is made cannot be what it is claimed to be, pure distilled city water.

"If it is distilled city water, there must be some great fault in the process of distillation or manipulation, or an exposure of the product to gross contamination, which not only falsifies the claim of purity for the water and ice, but may render the ice liable to be a source of serious ill health to the consumer.

"Whether this condition of affairs exists through ignorance or dishonesty we do not know and have no means of judging. But we deem it our duty to inform you of a state of things which seems not only liable to do harm in itself, but to undermine public confidence in general in the more successful attempts to produce and market pure water and ice."

On receipt of this letter a thorough investigation was ordered and the following report was made by the chemist of the board of health:

"SIR—I have the honor to report that I inspected the factory of the Hygeia Ice Company, and took the following samples:

"No. 1. City water, from tap in building where ice is frozen.

"No. 2. Distilled city water, from tank set in brine for the purpose of freezing.

"No. 3. Ice made from distilled city water.

"Nos. 4 and 5. Samples from ice cake in which about nine-tenths of the cake was clear (sample No. 4); about one-tenth was of a dark yellow color, containing much sediment (sample No. 5). Samples of all the above were taken for biological examination.

"For the chemical analysis the samples of water were taken in

clean demijohns, which had been previously washed with the sample of water to be taken. The samples of ice were taken in glass jars, which had previously been washed with city water, and were allowed to melt in said jars, the same being sealed during the operation. The analyses of these samples resulted as follows:

CHEMICAL ANALYSIS.

	1.	2.	3.	4.	5.
Appearance	Slightly turbid	Clear	Clear	Very turbid	Very slightly turbid
Color	Light yellow brown	Bluish	Bluish	Yellow brown	Greenish blue
Odor (heated to 100° Fahr.)	Marshy	Very faint	Faint	Faint	Tarry
Chlorine in chlorides	0.206	0.952	0.086	10.460	0.439
Equivalent to sodium chloride	0.339	1.569	0.141	17.240	0.724
Phosphates	None	None	None	None	None
Nitrites	None	None	None	None	None
Nitrogen in nitrates and nitrites	0.0165	0.0074	0.0088	0.0748	Trace
Free ammonia	Trace	0.0210	0.0130	0.0220	0.0030
Aluminoid ammonia	0.0080	Trace	Trace	0.0120	0.0070
Hardness equivalent to carbonate of lime: Before boiling	4.41	1.18	1.00	6.76	1.76
Hardness equivalent to carbonate of lime: After boiling	4.41	1.18	1.00	6.76	1.76
Organic and volatile (loss on ignition)	2.50	1.50	0.50	10.50	1.00
Mineral matter, non-volatile	5.50	7.00	5.00	40.00	5.50
Total solids (by evaporation)	8.00	8.50	5.50	50.50	6.50

"The samples of water for biological examination were taken in sterilized bottles. Of the ice, large pieces were taken to the laboratory. A small piece was cut from the interior of each of the samples, immersed in boiling sterilized water and transferred to a sterilized vessel, where it was allowed to melt.

BACTERIOLOGICAL ANALYSIS.

	1.	2.	3.	4.	5.
Bacteria per cubic centimetre	427	76	15	3,950	975

"From the chemical and biological examination of these samples I would conclude:

"First—That the sample of ice from discolored ice cake does not appear to be suitable for use in drinking water, or in any case where there is direct contact of the ice with articles of food or drink.

"Second—That the other sample of ice made from distilled city water is suitable for all domestic uses."

These letters show on their face that a bad condition existed, but, in my opinion, the conclusions of the chemist in the report do

not tell the whole story. By referring to the table of analyses it is shown that the distilled water was good, and that one sample of ice was good, but that the other sample not only was high in bacteria but also high in solids, and especially so in chlorine content, which indicates the direct introduction of animal sewage. The distilled water in the storage tank must have been good, and so the ice should have been good; so contamination must have taken place in the cans in the tank. As a matter of fact the can covers fitted badly and were in poor repair, and the stables immediately adjoined the freezing tank room. No proper facilities were given the employees for sanitary conveniences, and it is evident that animal and human excretions were conveyed to the covering of the tank and found their way into the ice cans.

By referring to the bacteriological examination of the raw water (No. 1), the distilled water in a can before being frozen (No. 2) and the ice frozen from this water (No. 3) a very interesting comparison is shown. The ice contained 80 per cent. less bacteria than the water from which it was frozen, and as the bacterial content of the distilled water was relatively low in comparison with the raw water, it is highly probable that the distilled water was sterile, but was put into a can which, at a previous freezing, had contained water contaminated by some outside cause.

The relative bacteriological content of plate and can ice is very well shown in a recent bulletin issued by the United States Government in regard to a systematic examination of can, plate and natural ice. The main object of the work described in detail in this bulletin was to find out the cause and prevalence of typhoid fever, and ice was considered as a possible channel.

In the bulletin it is stated, as a preface, that ice cannot be a frequent vehicle by which the infection of typhoid is spread, owing to the fact that the great majority of bacteria are killed in the process of freezing. The possibility, however, of typhoid infection being occasionally present in manufactured ice is indicated by the unclean methods found in use at the factories. Urine and excrement were shown to be carried on the shoes of the workmen to the tops of the cans or tanks, from there dropping into the freezing water. This was confirmed by the bacteriological examinations, which disclosed a greater number of organisms in the ice than in the water from which the ice was made. The contrary should be the case.

The can ice examined was made mostly from distilled water and should have been safe. The plate ice was made largely from

tap water, and, if not subsequently contaminated, should also have been reasonably free from injurious pollution.

A detailed description is given of the sanitary conditions surrounding the various manufacturing plants. In one plant, where both can and plate systems are used, the following sanitary conditions are noted:

"Wells, and Locations of Same.—Driven wells; all in yard between factory and stable, fence and spring room (basement). One is 80 feet deep; the other six are each about 200 feet deep. Spring, with very large volume of water, opens from foot of hill in east end of (an old, dark and muddy) unused basement room (full of trash, rotting wood and even horse manure).

"Employees, Water-closets, Etc.—There are seventy-five persons, including clerks and drivers, employed about the factory, foi whose use there is only one water-closet, over the office. (Insufficient, and men use yard, hillside, basement, etc.—anywhere they can get cover, as by weeds. Human feces on board cover to deep well near door to spring room.)

"Horses, Stables, Etc.—Fifty-one horses used for delivery of ice. Three stables in yard, between factory and hill, and very near seven driven wells and door to spring room. (Horse manure on and urine around board cover of nearest driven well.)"

This description will give an idea of the liability of contamination of the distilled water after it has reached the cans, and it shows that some very definite measures should be taken to prevent such conditions.

The report goes on to generalize on the condition of the plants, taken as a whole, and says that in no cases were the sanitary conditions as good as could be desired, and in some they were positively bad. In one place the can ice room had opening into it a window from a very filthy and ill-smelling urinal, which at once attracted attention.

In another factory the water used for ice making was taken from a spring which opens into a large unused basement room, and the water for condensing was obtained from wells in the narrow yard between the stable and the factory. This yard was rank with weeds. The board cover of the deep well nearest the stable was covered with horse manure and surrounded by horse urine. The board cover of the well nearest the open door of a very unsanitary basement room, into which a spring opens, was covered with newly deposited human feces, which was also noticed in the nearby weeds.

In a third place, the use of a very filthy, undrained and ill-

smelling stable in the centre of the factory building had just been discontinued. The stable was being converted into an ice-storage room. A new but undrained stable had been erected nearby. The board cover of a well in the corner of this stable, the water from which was said to be used for condensing, was covered with horse manure and surrounded by pools of horse urine. The yard between the stable and the factory was full of weeds, covered with horse droppings, very muddy and undrained at the time of inspection.

In many cases it was found that the wooden tops over the cans were dirty; that the cans and tanks were seldom, if ever, cleaned; that the men walked over the tanks after visiting the yards, stables, urinals, etc., bringing in filth or dirt on their shoes; and that water-closets were insufficient in number, inconvenient in place, and seldom if ever cleaned.

A table giving the bacteriological summary of these ice examinations shows as follows:

BACTERIOLOGICAL SUMMARY OF ICE EXAMINATIONS.

Number of specimens examined—	
Natural ice	6
Artificial ice	28
Total	34
Average number of bacteria per cubic centimetre—	
In natural ice	118
In artificial ice	123
Percentage of specimens of natural ice containing B. coli—	
In 1 c. c	16.6
In 10 c. c	33.3
Total	49.9
Percentage of specimens of artificial ice containing B. coli—	
In 1 c. c	14.3
In 10 c. c	14.3
Total	28.6
Average of nine plate ice specimens, bacteria per c. c	216
Percentage of plate ice specimens containing B. coli—	
In 1 c. c	20
In 10 c. c	10
Total	30

It is also shown in this bulletin that whereas the purifying effect of the process of freezing shows a marked improvement in the chem-

ical analysis of the ice over the tap water used, yet the bacteriological findings show the contrary in many cases.

Five specimens of plate ice, for example, contained the following comparatively high bacterial contaminations, whereas tap water from which it was made averaged only sixty-four organisms per cubic centimetre during the same period of time:

BACTERIAL CONTAMINATION OF PLATE ICE.

No. of Sample.	Organisms Per Cubic Centimetre.	Colon Bacillus.
24	455	Absent.
29	5,000	In 1 c. c.
26	230	In 10 c. c.
27	650	Absent.
34	470	Absent.

The discrepancy between the bacteriological and chemical findings doubtless signifies that the water in the tanks is contaminated by the unclean methods already noted in the inspection of the factories, and that, while the organisms are not entirely destroyed at the low temperature of the ice tank, they cannot effect those changes in water which they bring about at ordinary temperatures, and which result in the production of characteristic substances whose presence can be recognized and determined by chemical methods.

Specimen No. 29 is a striking example of this condition. This ice contains 5,000 bacteria per cubic centimetre, with the colon bacillus present in at least each cubic centimetre. On the other hand, the chemical analysis of this specimen does not indicate injurious pollution. It would seem reasonable to conclude, therefore, that the bacterial contamination had probably dropped into the water from the soiled shoes of the men working on the ice, or from other sources just before freezing.

The facts presented here, and my own tests in many plants, show that lack of a true knowledge of sanitation causes, in some plants, contamination of ice as delivered. I have shown by a series of exhaustive tests, which I published a few months ago, that temperature has a very marked effect on the pathogenic organisms that may be met with in natural ice cut from polluted water.

Natural ice, in nearly every case which has come under my observation, when harvested contains bacteria. This is due to the fact that any open surfaces of water suitable for cutting ice are liable to receive surface drainage. When this ice has been stored for some time the diminution of bacterial content is very marked. In my tests on characteristic sewage bacilli I found that the entire elimination of the B. coli communis, at a temperature of 28° Fahr., took twelve weeks; that the entire elimination of typhoid bacilli took,

on an average of three different tests, nine weeks; that the entire elimination of sewage streptococci took twenty weeks at 28° Fahr.; and at the same temperature it took twenty weeks to eliminate the anaërobic and pore-forming bacilli. It is interesting to note that in the Government tests recorded above, the determinations of which were made in midsummer, when the natural ice had been stored for many months, and the can and plate ice was being consumed as quickly as it was made without any chance of storage, the average bacterial content of the different samples of ice examined showed that the manufactured ice, taking can and plate together, contained 123 bacteria per cubic centimetre, while the natural ice contained 118 per cubic centimetre, and that the average of nine samples of plate ice alone showed an average of 216 bacteria per cubic centimetre. This shows very conclusively not only that storage of ice has a great value, but that contamination of manufactured ice takes place in the freezing tanks.

I do not wish to be understood as waving a red flag and saying that all ice is contaminated. It is not. Some of it is, however, and the ice that is contaminated gets in that condition because of the lack of due consideration of sanitary details by the designers of the plant. If the manufacturer of the machinery, tanks, etc., used in an ice-making plant contracts to deliver these of a definite size and capacity, his responsibility ends with a proof as to their efficiency. If, on the other hand, he contracts to put in a plant complete in every detail, the sanitary conditions of the plant should receive due attention at his hands.

It is, in my opinion, the duty of the manufacturers of refrigerating machines and the consulting engineers to provide for their clients not only a definite quantity of ice per day, but good, salable ice. To produce good, salable ice, not only must the ice appear clear, by efficient reboiling to remove the air in the can system, and efficient air agitation in the plate system, but also the sanitary condition of the ice must be given careful consideration. It is easy, by mechanical means, to make ice that is pleasing to the eye and which will be readily accepted at full price on the platform by the retailers, but the data I have given show very conclusively that this is not enough.

I am fully aware of the fact that a just criticism might be made in regard to the responsibility of the manufacturer of the refrigerating machinery used in an ice plant being limited only to the quantity of ice produced, and that the quality of the ice is up to the operator of the plant, for the reason that, at present, even when a com-

plete plant is put in, the details as to the design and arrangement of the remaining portions of the plant, not essential to the manufacture of ice, are arranged for and provided by the actual owner of the plant who is to operate it, either personally or through his architect. In very few cases that have come within my knowledge have the sanitary details been a duty or even a consideration of the installer of the refrigerating machinery. I wish to go on record as stating that although the general precedent and custom is for the manufacturer of the machinery to consider that this is none of his duty and not part of his obligation, yet it really is. I do not agree with this contention, as it has a marked bearing on the quality of ice in which he is directly interested, under the clause of being able to deliver by his plant a certain quantity of good, salable ice per day. This "good, salable ice" must be what the words mean in their broadest sense, and to accomplish this end it is necessary for him not only to suggest the necessary sanitary conveniences which should be incorporated into the plant as a whole, but he should also notify the operator of the plant that certain sanitary precautions should be taken, so that the ice may really be salable from every point of view. What I wish to bring to your attention is that to secure good ice, consideration of the sanitary conditions and facilities in the plant is just as necessary as attention to the reboilers, air agitators, or any other method used in making the ice free from cloudiness, and although, in the past, you have not considered this your duty, yet you must realize that this is part of your obligation in the future.

An ice plant has enough troubles of its own to contend with without running into a snag like a quarrel with the local board of health. The sarcastic letter of the pathologists I have quoted in regard to "the so-called Hygeia Ice Company" would do more harm to the average ice plant, if it became public, than could be made up by years of work.

In regard to preventing the contamination of the distilled water in the cans of a can plant and the water in the freezing tanks of a plate plant, a few precautions in the design and operation of the plant are necessary. The necessary conveniences and wash rooms should be provided according to the size of the plant and the number of men required. These should be on the ground floor at some point away from the tank room. The floors should be of tile, set in concrete, so that they can be washed off with a hose and kept clean. The stables should be so placed that they have an independent entrance from the street and the yard of the plant is not used for this purpose. The loading platform should be on the street and not

on an interior yard. All wells or other sources of water supply should be protected from surface contamination.

In regard to preventing the contamination of ice manufactured by the plate system, the precautions just mentioned would apply in a can plant to the general details of the plant. In addition the water, especially when taken from wells and springs on the property on which the ice plant is located, should be filtered by some means of filtration of high efficiency, both as to matter in suspension and bacterial content. Filters of high efficiency can be obtained so that it is possible to insure that the water entering the tanks is free from contamination. If contamination of the water after it gets in the tanks is prevented, as outlined above, good sanitary ice should be made.

In regard to the operation of the plants, the can and tank covers should be cleaned often enough to insure cleanliness, and, in a plate plant, the tanks should occasionally be cleaned. The can and tank covers should be kept in good repair and be replaced as soon as possible after being lifted. No one should be allowed on or about tanks or cans except those employed there. The tank men should wear rubber boots which can be washed off, and which should be left in the plant when the day's work is over. The employees in the ice plant should not be allowed to enter the stables, and no roustabout should be employed spending part of his time in the stable and part on the freezing tanks. All urinals, etc., should be kept scrupulously clean, yards and surrounding open spaces nearby kept clear of weeds, and penalties be imposed for defecating in any of the parts of the plant, yard, stables or surrounding open spaces. All empty rooms should be kept clean and closed when not used.

Now that local boards of health and the United States Government have begun to investigate manufactured ice, and because, in the near future, we may expect a food inspection decision report on ice which will lead to close restrictions as to the sale of this product, I consider that it is the duty of every manufacturer of complete ice-making plants, and every refrigerating engineer, to insure their clients against unsanitary ice by inserting the proper facilities and issuing the proper instructions to keep the ice free from contamination.

DISCUSSION.

The President.—The paper by Mr. Sparks is an education in sanitation. It would not be a bad plan if everyone operating an ice plant had a copy of it.

George Berna.—Mr. President, as a man who has been running

a small ice-manufacturing plant for two seasons, I desire to say that I employ a watchman and have him scrub the tank cover every night, particularly that part we walk over. The can covers are made of hardwood and I had them painted white, so that the man can see they are clean. The concrete walls of the tank room are whitewashed, and all other wood work or iron work is painted white, so that if there is any dirt we can see it and remove it. I do not know of any better plan of keeping a plant clean than by having it painted in such a manner that you can practically determine this point on all occasions. The wooden covers, as they are used in the majority of plants, should be either filled and then painted, or, anyhow, made smooth and painted. I have found that a great many covers are too rough; splinters come off from the covers and framing, particularly where riveted cans are used. The rivets on the cans act like a saw on the frame, and every time the can is put down they cut off a lot of very small chips. An improvement would be to put a strip of flat rubber over the frames for the covers to rest on. The only other way is to let the cans down on the flat side, so that the rivets will not touch the frame. This idea was followed as long as seventeen years ago in New Haven, Conn. The people asked: "What shall we do with our plant? How shall we paint it?" I said: "Paint it white. It is, first, the best to reflect heat and keep things cool, and, second, to keep it clean. You will always know whether it is clean or not."

John C. Sparks.—Mr. President, would Mr. Berna tell us if he used the man to harvest the ice only, or for any other operations?

George Berna.—Yes, I only kept this man for harvesting the ice. We have one difficulty, and that is at night, when he is alone, he has to go to the boiler room, and so he has to be very careful about carrying coal dust into the tank room. The closets and such things are about 100 yards away from the tank room and buildings. A notice is put up stating that nobody else is allowed on the tank. We have signs up requesting visitors to keep away from the tank. We do not allow drivers, who may carry horse manure on their shoes, to come from the yard into the tank room, and certainly not on the tank top. We have the house closed and the platform is washed previous to taking ice cakes over it.

John C. Sparks.—I believe that the unsanitary condition of manufactured ice is due to ignorance and not to anything else. Everybody knows that the ordinary water-closet is a very uncleanly place. In one's home the water-closet never gets out of repair, but in an ordinary plant it is very different. The average ice harvester is

an uncleanly animal and needs looking after or he puts the toilets out of commission, as is said. In a plant where there were seventy-three men using one water-closet this one closet was neglected and dirty, and was not in working order.

George Berna.—There should be enough water-closets and the owner should keep them locked and a key in a certain place. We have only four men, two in the day and two at night, and two water-closets outside of the building.

Harold W. Cole.—I was afraid, perhaps, that there might be some objection to Mr. Sparks' paper on the ground that perhaps the United States authorities that he quoted, or he himself, had selected some few strikingly bad plants from a sanitary standpoint and was holding these up as a horrible example. But that point has been noted, and the few remarks that I had in mind to make in corroboration of that are perhaps not called for. But I do want to say that in the course of the last few years I have had occasion to go into a good many plants, and to the casual observer there are undoubtedly more plants where no sanitary precautions are taken at all than there are those in which even the most ordinary and most soon thought of precautions are rigidly enforced. There was one plant within the limits of New York city doing a large business selling ice at retail to families and supplying office buildings, which I went through and found conditions practically the same as those to which Mr. Sparks' paper referred. There was a stable in the yard adjoining the room in which the ice tank was situated, and between the tank building and the stable building was an immense pile of manure, and the men passed back and forth over this, so that there were lying on top of the covers of the cans pieces of horse manure and straw. The men who were walking over the tanks and pulling the ice had their boots covered with small particles of undigested oats and pieces of straw, and this was right in the heart of New York city. So I think that there is really a very pronounced necessity for action on the part of the designers of refrigerating machinery and refrigerating engineers to assist the general public in getting a little better ice than that with which they are quite frequently served.

John C. Sparks.—I think part of this unsanitary condition is because, as plants are designed at present, the cover of the freezing tank is a highway to get from one part of the plant to another, and the workmen have to walk over the top of the ice cans. There is no space on the sides that can be used as a passage, and the top of the freezing tank seems to be a place to walk about anywhere you like, the shortest way to anywhere. This certainly should not be. There

should be another passage clear of the tanks. This is directly part of the design of the plant and up to the man who lays it out.

The President.—It occurred to me in listening to this paper that one solution would be to seal the cans. If the cans are filled from the source of supply, the distilled water tank, there is no earthly reason why they should not be covered, and this would be especially necessary where conditions are so unsanitary, where there would be contamination of the contents of the can if it remained open.

John C. Sparks.—You mean a special cover for the can?

The President.—Yes. You might have to have some other way to protect the pure water in the can. The process of freezing does not add to the bacteriological contents.

Gardner T. Voorhees.—I think Mr. Sparks' paper is one of the most interesting and valuable papers that have been presented on ice-making, and I think it is of such vital interest to ice makers themselves that I wish some one of the Society could suggest some way that this could be put before the owners of ice plants, so that they could realize its importance, as Mr. Sparks puts it. That is, when this comes out in a written paper by Mr. Sparks—you know how we often read these papers: we look at the front page and the end, missing the most important part. Now, I believe, lots of people will lose the most important part of that paper by Mr. Sparks. I do not think, as refrigerating engineers, we want to get the public too keen on that, because they may go after ice plant owners in an injurious manner. I think the owners of ice plants should go after it themselves, and I wish there was some way that this Society could convey in a few words to the owners of ice plants the great value to them in what Mr. Sparks has brought out.

George Berna.—I think, Mr. President, this is very necessary, because the man who sells a plant, also the contracting engineer who puts it up, has his hands full, and is called home after he is done, or does not attempt to call these sanitary conditions to the owner's attention. The only chance the refrigerating engineer has is in a case where he is called in, and then you might say the thing is already spoiled to some extent. I also want to add that some attention will have to be given to the distilled water storage tanks, as you probably will find some plants where they are not covered. I want to ask Mr. Sparks whether there is not a danger of bacteria coming in in this way as well as directly into the freezing tanks.

John C. Sparks.—The difference between bacteria that are carried in air and bacteria that are carried in water is that one is called aërobic and the other is called anaërobic. The bacteria that you get

in air are not in their natural condition in water, and they do not propagate and do not increase, and therefore the bacteria in the air would not affect ice very much. I know of some distilled water storage tanks where the water is rotten and the ice tastes like gum shoes; but this is because algæ were in the water, and some of the undistilled water was allowed to get in, and the temperature of the storage tanks was just about right and the water was awful. But I do not think from an actual sanitary point of view, from the point of view of intestinal germs—and we are only considering the possible evidences of disease—that ice can get harmful bacteria from the air. The pure food act demands that in spring waters the water should be as good when it is delivered to the customer as it is in the spring, therefore the bottler cannot put the water in a dirty bottle and call the water "pure spring water," as a dirty bottle may and often does contain intestinal germs. In regard to ice, the bacteria are water-born germs, not air-born germs, and so it is really not as important.

George Berna.—What made me ask the question is, while in Europe some years ago some people told me not to open the windows, as the dust from the street made people get sore eyes. Last summer some of the farmers in Tompkins County, New York State, got what they called dust fever from the threshing machines. So I thought that probably there was some danger from contamination from dust, too.

John C. Sparks.—I did not know you included dust in your remarks. Of course, if you have dry excretion of any sort containing pathogenic bacteria, those germs will be revived in the distilled water. I was just considering contamination from air, and not dust containing bacteria. It is just as bad on the ice if you had fecal matter dried and then blown in from the street.

No. 52.

ETHYL CHLORIDE REFRIGERATION.

By C. C. Palmer, New York, N. Y.
(Member of the Society.)

In introducing the subject of ethyl chloride refrigeration, I desire to state that there are very little data available upon which a technical paper can be founded. However, I give such data as I have at hand and the results of several years' experience with ethyl chloride as a refrigerating agent.

Ethyl chloride, which I call "clothel," is the monochloride of ethyl (C_2H_5Cl). It is the product of ethyl alcohol or grain alcohol treated with hydrochloric acid gas. It is a colorless liquid and its specific gravity is 0.925 at 23° Fahr. It boils at an atmospheric pressure of 54.5° Fahr. Its critical temperature is 365° Fahr., and its latent heat is 174 B. T. U. at 23° Fahr. It is a neutral chemical, and when properly made will show but the slightest trace of acid upon litmus paper. It is neither deleterious nor obnoxious, and should it be necessary at any time to repack the valve stem or the stuffing box of the compressor no inconvenience whatever will be experienced from escaping gas. It possesses nothing in common with either air or water; consequently, should either of these elements be introduced into the system by accident, no chemical change would take place. As the critical temperature is high, there is no generation of permanent gases to impair the efficiency of the refrigerating system, and from my experience it does not deteriorate in value as a refrigerating medium. It is apparently just as good and just as efficient after a year's use as when first introduced into the system. Its boiling point being high, namely 54.5° Fahr., it has a correspondingly low point of liquefaction under pressure, namely, 15 pounds per square inch, gauge pressure, with condenser water at 65° Fahr. Its pressure being low, the loss of chemical in action from leakage is reduced to a minimum. Its low pressure, together with it being a neutral chemical, insures a long life to the machine and little liability to accident. It is not, however, applicable for sharp freezing or temperature of zero or below, but it is a favorable chemical in the manufacture of ice and in the production of ordinary cold storage temperatures. In my opinion ethyl chloride approaches nearer to the ideal

chemical for refrigeration than any of the chemicals now in use, and I predict for it a prominent place in this field in the future, not only on account of its stability and the ease with which it can be handled in a machine, but also from the fact that in the very near future it will be manufactured from denatured alcohol, free from Government tax, which will make its cost practically the same as several of the other chemicals used in mechanical refrigeration.

I give herewith the vapor pressures, etc., from which computations can be made as desired:

"CLOTHEL" (ETHYL CHLORIDE)—TEMPERATURES (FAHRENHEIT) AND PRESSURES (ABSOLUTE) IN POUNDS PER SQUARE INCH.

(Translated from Regnault's Tables as given in "Smithsonian Physical Tables." Gray, 1897, page 127.)

Degrees Fahrenheit.	Pounds, Absolute.
—22	2.18
—18	2.94
—4	3.71
5	4.74
14	5.98
25	7.45
32	9.21
41	11.27
50	12.10
59	16.48
68	19.72
77	23.44
86	27.70
95	32.53
104	38.00
113	44.16
122	51.06
131	52.82
140	67.32
149	76.78
158	87.20
167	98.64
176	111.14
185	124.75
194	139.52
203	155.48
212	172.63

Specific heat, liquid ..... .4276
Specific heat, gas ..... .272
Specific gravity, liquid ..... .925
Specific gravity, gas at 0° C. (air being 1) 2.219
Latent heat, liquid, 174 B. T. U. at 23° Fahr.
Critical pressure, 53 atmospheres.
Critical temperature, 365° Fahr., or 185° C.

To save those interested the trouble of referring to the technical libraries, I give below tables of vapor pressures of some of the chemicals used in mechanical refrigeration, as given in "Smithsonian Physical Tables," 1897. All are from Regnault's results, except Pictet's fluid. Ethyl ether (sulphuric ether) is still used to a limited extent in India and the tropics on account of its low pressure. "Clothel" is more powerful and is adapted to any tropical climate:

VAPOR PRESSURES OF VARIOUS REFRIGERATING AGENTS (PRESSURES IN MILLIMETRES).

Temperature Centigrade, Degrees.	Ammonia, NH_3.	Carbon Dioxide, CO_2.	Pictet's Fluid, $64SO_2$ $44CO_2$ by Weight.	Sulphur Dioxide, SO_2.	"Clothel," Ethyl Chloride, C_2H_5Cl.	Ethyl Ether, $C_4H_{10}O$.
—30	86.61	.....	58.52	28.75	11.02	.....
—25	110.43	1300.70	67.64	37.38	14.50	.....
—20	139.21	1514.24	74.48	47.95	18.75	6.89
—15	173.65	1758.25	89.68	60.79	23.96	8.93
—10	214.46	2034.02	101.84	76.25	30.21	11.47
—5	264.42	2344.13	121.60	94.69	37.67	14.61
0	318.33	2690.66	139.08	116.51	46.52	18.44
5	383.03	3075.38	167.20	142.11	56.93	23.09
10	457.40	3499.86	193.80	171.95	61.11	28.68
15	543.34	3964.69	226.48	206.49	83.86	35.36
20	638.78	4471.66	258.40	246.20	99.62	43.28
25	747.70	5020.73	297.92	291.60	118.42	52.59
30	870.10	5611.90	338.20	343.18	139.90	63.48
35	1007.02	6244.73	383.80	401.48	164.32	76.12
40	1159.53	6918.44	434.72	467.02	191.96	90.70
45	1328.73	7631.46	478.80	540.35	223.07	107.42
50	1515.83	.....	521.36	622.00	257.94	126.48
55	1721.98	.....	.....	712.50	266.84	148.11
60	1948.21	.....	.....	812.38	340.05	172.50
65	2196.51	.....	.....	922.14	387.85	199.89
70	2467.55	.....	.....	.....	440.50	230.49
75	2763.00	.....	.....	.....	498.27	264.54
80	3084.31	.....	.....	.....	561.41	302.28
85	3433.09	.....	.....	.....	630.16	343.95
90	3810.92	.....	.....	.....	704.75	389.83
95	4219.57	.....	.....	.....	785.39	440.18
100	4660.82	.....	.....	.....	872.28	495.33

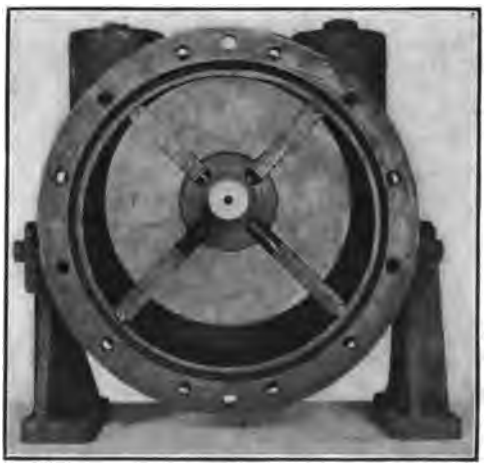
THE MACHINE.

Ethyl chloride belongs to that class of chemicals in which the work must be done under partial vacuum, and, consequently, the machine must be correctly proportioned and the compressor so constructed that there is no possibility of air being taken into the system.

The ordinary reciprocating compressor can be used, and a number of our early experiments were made with this form of com-

pressor, the stuffing box around the piston rod being sealed with glycerine for the purpose of preventing air being taken into the system; but it was left to the engineer to see that the glycerine seal was always maintained. If by accident the glycerine was allowed to be drawn into the system air would soon be introduced and the efficiency of the machine very materially impaired.

In our work we use a rotary compressor, consisting of a cylinder having within it a spider or drum set eccentrically to the centre of the cylinder. The top of the drum comes in close contact with the upper side of the cylinder and contains sliding blades, which, in their rotation, follow the gradually increasing space between the drum and the cylinder wall and maintain their close contact with the cylinder



CROSS-SECTION OF "CLOTHEL" ROTARY COMPRESSOR.

wall, producing the vacuum necessary in vaporizing the chemical. After passing the bottom centre, where the space is the largest, the space becomes gradually contracted, the blades receding into the drum until they reach the top of the cylinder, where, as mentioned above, the drum comes in close contact, causing the gas under pressure to be expelled from the cylinder through ports provided for this purpose. No difficulty is experienced in maintaining a 20-inch or 25-inch vacuum, and at the same time sufficient pressure to liquefy the gas, even with condenser water at 90° or 100° Fahr. The general construction of this compressor is undoubtedly known to you all.

An important fact in the operation of this compressor is that the work on both the vacuum and the pressure side is done entirely on the cylinder wall, the centre or axis of rotation being in almost a

neutral zone. There is, however, a slight excess of pressure over vacuum, so that there is always an outward pressure, which insures against the introduction of air into the system and also forces the lubricant into the bearings of the compressor. We interpose on the discharge side of the compressor a trap or separator by which the lubricant is separated from the gas and in turn returned to the bearings of the compressor, and from there to the sliding blades, making a continuous circuit of the lubricant.

This compressor is made without valves, and it is so constructed that no part can become misplaced to cause accident or retard its work. There is another point in its favor, namely, that for use where electric current is the source of power we have a very small backlash. In the smaller machines it is hardly perceptible, and thus insures to the electric motor a uniform and even load, which is very desirable from an electrical engineering standpoint.

We formerly used seamless drawn copper tubing to a large extent in the construction of our condensers and refrigerator piping. The high price, however, of copper has caused us to abandon its use, and we are now using a light seamless drawn steel tube. The refrigerator piping is usually made in the form of grids, with manifold at top and bottom. The pipes connecting the manifolds are welded into the manifolds, making a construction that is exceedingly desirable and retains practically 90 per cent. of the tensile strength of the tubes in the joints so welded, and at the same time, by the use of these light tubes, ordinarily of 18 gauge, we are obtaining an efficiency closely approximating that of the copper.

Having a neutral chemical to deal with, we can, in the matter of gauges, valves, etc., purchase considerable of our material in the open market. Our expansion valve, however, is of special design, and while not applicable to the requirements of the higher pressure chemicals, it has proven to be extremely efficient in the handling of ethyl chloride. We find that it is desirable in the construction of our machines to use a trap or pocket in which to collect the excess of the lubricant or any fine particles of foreign matter that might pass the separator into the condenser.

The parts described, together with the connecting pipes, gauges and valves, complete the description of the machine, which is exceedingly simple in its construction, and little or no skill is required in its operation.

Where electric motors are used we open and close the water supply to the condenser from the electric switch, the amount of water necessary being regulated by a fixed valve in the circuit.

We find that there is an advantage in working this chemical under a vacuum, and by setting the expansion valve in its proper place the machine can be operated continuously, so that the result in the storage room will be almost a perfectly uniform temperature. The vacuum gauge is our source of information as to the quantity of chemical required each hour to produce the desired results. The higher the vacuum the lower the degree in which the chemical is being boiled or evaporated, consequently the greater the degree of cold produced.

The introduction of electric power and light in all of the large cities and a majority of smaller ones furnishes the power for operating small refrigerating machines and opens a field of almost unlimited extent. Our experience has been that the electric companies encourage the installation of this class of machinery, as it furnishes a new field for the sale of power, and at a season of the year when the lighting load is at a minimum. It is my opinion that it is more desirable to operate a machine continuously with a fixed low power current extending over several hours than to run it intermittently on a higher power at indefinite and unknown hours of the day. It is obvious that practically the same amount of power must be used to produce a given result, whether it is of a fixed degree, distributed uniformly over the several hours of operation, or whether it is a more intense current and used a shorter length of time and intermittently. We have, therefore, proportioned our machines for the method which we believe most advantageous to the operator and the electric companies, that is, operate the machine as far as practical during the day time, and have a storage of cold brine to hold the temperature during the time the machine is not running.

DISCUSSION.

Albert Johnson.—What pressure has the outgoing gas?

C. C. Palmer.—With condenser water at 65° Fahr., the gauge pressure of the discharged gas is about 15 pounds to the square inch. Of course, if the temperature of the condenser water increases your pressure increases in proportion.

Albert Johnson.—How do you keep the refrigerant from passing the blades?

C. C. Palmer.—The face of the cylinder spider and blades is a perfect plane. The drum is made to within about three or five thousandths of an inch of filling the entire space between the cylinder heads. The excess of lubricant between the head of the cylinder and the drum makes a seal and prevents by-passing anywhere.

Albert Johnson.—I asked that because I had a water pump just

like it in my automobile; just the identical thing, only I had springs under each blade. I could not use the pump after a while, as the slippage was so great that I was not pumping anything, and I had to do away with it.

C. C. Palmer.—Our experience has been that the wear on the cylinder heads is insignificant, and the wear on the whole thing has been very slight. The space which we provide, of three or five thousandths of an inch, is sufficient, so that there is no what you might call fore-and-aft pressure. The entire pressure in the operation of the machine is on the walls of the cylinder, on the outer circle.

Albert Johnson.—It is all open.

C. C. Palmer.—We have no end pressure at all. There is nothing to cause an end pressure. That is, the pressures are, strictly speaking, on the shaft. It is between the drum and the cylinder wall.

Albert Johnson.—I do not see how it is going to work unless you provide some means for keeping the 15 pounds pressure gas from going back into the vacuum chamber on the other side of the blades.

C. C. Palmer.—Well, experience has told us that by making it with that clearance of three- or five-thousandths of an inch, and with the lubricant, there is no wear, so that after running a year or two or three years we have still a perfectly efficient machine. The wear on the blades is taken up by a half round strip of soft steel. The pressure keeping the front blade in contact with the cylinder walls, the lubricant making a perfect seal. We can maintain a 20-inch vacuum on one side and a 15 or 20 pound pressure on the opposite side. I would not recommend it for high pressure work, 200 or 300 pounds. In your case I do not know the exact nature of your machine, but grit or something else caused the wear.

The President.—I might ask Mr. Palmer about how large is the diameter of the bore of the cylinder, the largest that is made?

C. C. Palmer.—About 16 inches is the largest that we make.

The President.—What is the width of the blade?

C. C. Palmer.—About 14 inches.

The President.—What is the number of revolutions at which you run it?

C. C. Palmer.—We run this about 150. The smaller ones we run up as high as 300.

The President.—What would be the refrigerating capacity of the 18-inch machine?

C. C. Palmer.—About 5 tons.

The President.—What power does it take to drive it?

C. C. Palmer.—About 10 to 15 horse power. Of course, the

amount of power depends on the temperature of the condenser water. Our effort is to build small machines from 250 pounds up to about 5 tons refrigerating capacity.

The President.—This paper is interesting, as it gives information of another liquid that can be used in refrigeration.

Charles E. Lucke.—I have tested a good many rotary steam engines and several of this type, some almost identical in construction. The experience with them has been pretty nearly uniform, to the effect that the friction was high and the leakage great, and increasingly great with age, and that any attempt to reduce the friction resulted in greater leakage, and any attempt to reduce the leakage resulted in greater friction. I was wondering whether Mr. Palmer had any figures available along similar lines with this type of machine used as a low-pressure compressor to show exactly how much leakage it suffered, how much power it took to drive it, and how both of those things vary with age.

C. C. Palmer.—We have no data that would give the exact comparison. Our experience has been that the friction in the rotary compressor is a little larger than it would be in a reciprocating compressor, but in small machines it is so small that it is really no great detriment to it. Now, on a 100-ton machine the friction factor would be entirely different. Our experience has been with small machines, where the friction factor is not so important. Still, when we come to figure the friction of the stuffing boxes on the reciprocating compressor and then offset that by the friction we have, the percentage will not be excessive. It may be slightly more in our apparatus than it will in the other. Regarding the wear, our experience has been that we have had these compressors running for several years and the efficiency has continued good.

Charles E. Lucke.—I measured the friction of a very small reciprocating compressor, with a very small bore, not bigger than your wrist, and found the friction to be about 100 per cent.; that is, the power used in actual compression was just about equal to the power lost in friction.

C. C. Palmer.—What made me make the remark was that when you pack stuffing boxes for the high-pressure chemicals it raises the friction factor about as high as we get from end thrust.

The President.—What do you use for a lubricant?

C. C. Palmer.—Glycerine.

The President.—You run at a moderate speed?

C. C. Palmer.—Yes.

The President.—And there is no springing of the cylinder or heads of the machine itself?

C. C. Palmer.—No.

The President.—With the lubricant freely interposed between surfaces that construction might run for a long period without repairs?

C. C. Palmer.—Yes.

The President.—Of what material are these packing strips?

C. C. Palmer.—Soft steel. The whole interior construction is of soft steel or cast iron.

The President.—What would be your working pressure?

C. C. Palmer.—From 15 to 22 pounds per square inch, gauge pressure.

The President.—I will have to admit that conditions seem to be favorable for that sort of a machine.

C. C. Palmer.—We are not advocating it for high-pressure chemicals, but for low-pressure chemicals, and for the uses for which we have designed it we believe it to be all that we have claimed for it.

The President.—Does the shaft extend through both heads?

C. C. Palmer.—Yes, they are packed. We use babbitt bearings.

D. S. Jacobus.—Do you flood the bearings with glycerine?

C. C. Palmer.—Yes. The lubricant comes from the separator to each of the bearings, and from there toward the cylinder walls and then out on the blades, which gives a perfect circulation of the lubricant.

George Berna.—Have you ever thought of using a centrifugal compressor?

C. C. Palmer.—I doubt if you can use a centrifugal pump upon our gas, where you must maintain on one side a partial vacuum and on the other a pressure to insure the expulsion of gas from the cylinder.

R. L. Shipman.—I would like to ask the gentleman what he means by the centre or axis of rotation being in almost a neutral zone.

C. C. Palmer.—The axis of rotation is almost in the centre of the cylinder. We have on the pressure side, for instance, 15 or 18 pounds pressure, and on the other side a vacuum. Now, between the point of contact of the outer circle of the drum and the shaft we have it practically closed—that is, it is almost in perfect contact; the excess of pressure at 15 pounds, over a vacuum of, say, 20 inches, would be 5 pounds outward pressure.

R. L. Shipman.—Five pounds?

C. C. Palmer.—Yes; that would give us in the central point an outward pressure instead of a suction.

R. L. Shipman.—Do I understand that there is no side thrust on the shaft?

C. C. Palmer.—There is no end thrust.

R. L. Shipman.—That may be. There is a side thrust, of course?

C. C. Palmer.—Yes, all the work is done on the outer circle or cylinder walls. There is practically no end thrust on the heads.

R. L. Shipman.—But there is a side thrust in the same direction of about 25 pounds to the square inch through the centre section of the cylinder?

C. C. Palmer.—Only 15 pounds inward pressure, but equivalent to 20-inch vacuum on the other side.

R. L. Shipman.—Did you say that the blades are held apart by a rigid pin?

C. C. Palmer.—Yes.

R. L. Shipman.—That is always against the blades?

C. C. Palmer.—Yes.

R. L. Shipman.—Now, when the blades stand in a vertical position, is not the distance greater between the ends of the blades than when they are in a horizontal position?

C. C. Palmer.—No.

R. L. Shipman.—Is not the diameter in the vertical position the diameter of the cylinder itself?

C. C. Palmer.—No, the cylinder is bored out of round on purpose, so that the length of a line drawn through the center of the axis of rotation horizontally is exactly the same as that of a line drawn through the axis vertically.

R. L. Shipman.—It must be a very expensive construction. It is not a true cylinder?

C. C. Palmer.—It is not a true cylinder, no. It is bored out on purpose to compensate for the travel of these blades, so that they are always in perfect contact with the periphery of the circle.

R. L. Shipman.—Did you say that the machine is operated without valves?

C. C. Palmer.—Yes.

R. L. Shipman.—I think the machine would operate with less power if it had a valve on the discharge side.

C. C. Palmer.—That possibly may be so.

R. L. Shipman.—When the blade passes the discharge port, and when it begins to open the discharge line to the other blade, then

the pressure above that blade, which is just opened to the discharge line, would rush back on the other side, and we would have what is commonly called a "chewing" of the discharged gas.

C. C. Palmer.—The outlet port is carried in from the top of the cylinder a quarter of the way down. As each blade comes around there is no point at which the gas on the pressure side of the cylinder can be communicated to the opposite side.

R. L. Shipman.—But there is a point when the blade coming up will communicate the gas to the opposite blade going down.

C. C. Palmer.—Between the top of the cylinder and one-quarter of the circle down there will always be a uniform pressure, because the following blade makes it. For the entire length of the port the pressure is exactly the same the minute the blade passes the lower opening of the port.

R. L. Shipman.—Then that blade must not be doing any work from the horizontal position to the vertical?

C. C. Palmer.—There is some loss, but we compensate for that by the use of four blades in the machine.

R. L. Shipman.—How many horse power do you require per ton in your machine?

C. C. Palmer.—We figure usually about 2 to 3 horse power per ton.

R. L. Shipman.—I have found in machines of that size that it usually required 2 or 3 horse power per ton.

C. C. Palmer.—That depends on the temperature of your condenser water.

R. L. Shipman.—But we are assuming the same conditions and circumstances. That puts both machines on the same basis.

C. C. Palmer.—Yes.

R. L. Shipman.—It looks to me as if your machine will work with more friction than the ordinary reciprocating machine.

C. C. Palmer.—I said it has a little more friction than the ordinary reciprocating machine.

D. S. Jacobus.—Professor Lucke has already mentioned the fact that the friction of the rotary steam engines is excessive and that they soon wear so as to cause a large leakage. I have, however, had some experience with a rotary vacuum pump and have found that it may be made to give better service than a rotary steam engine. I know of one such pump that was run at intervals for fifteen years. At first it had springs to hold out the blades, but we took the springs out, and it ran just as well without them, because the centrifugal force held the blades in position.

C. C. Palmer.—That has been our experience after six or seven years of use. The wearing of the blades and the cylinder wall is taken up by the half-round piece of soft steel at the end of each blade. There is little or no end thrust, consequently there is little or no wear there.

George W. Wright.—What is the cost of the refrigerating fluid, ethyl chloride?

C. C. Palmer.—It costs 75 cents a pound. As soon as we get tax-free alcohol it will probably cost in the neighborhood of 20 or 25 cents per pound.

The President.—Let me ask Mr. Palmer if the gas is inflammable.

C. C. Palmer.—No; not in that sense of the word. The chemical has been passed upon by the National Board of Fire Underwriters, and we are permitted to put it in any building without any additional insurance. Doctor Jacobus can probably tell us something of that.

D. S. Jacobus.—I made a number of experiments at one time to cover this point. We built the equivalent of a small room with loose sides and set it up in a vacant lot, filled it with various mixtures of the gas and air, and stood off at a distance to see what happened when the mixtures were ignited. We found no evidences of explosions. We then made openings from about 2 inches to 6 inches in diameter and covered them with tissue paper, to see if we could note any explosive action. The tissue paper was blown from the larger openings in some cases, but not from the smaller openings, these showing that the explosions were of a very mild nature. There is not very much force to it.

The President.—I might ask Doctor Jacobus if he made a test under conditions where there was air surcharged with gas and lubricating oil.

D. S. Jacobus.—We tried lubricating oil in the gas. In some of the experiments we strove to obtain a uniform mixture. In others we placed a lamp in one corner and let the gas creep toward the flame. We tried in many ways to produce a severe explosion, but did not succeed.

The President.—I might make the remark right here that many an experiment, such as Doctor Jacobus has described, outside of the hands of an expert, has killed men; that the thing does not happen when it ought to happen; but when it is put out in the market, and Tom, Dick and Harry get hold of it to run it, then things do happen. The most of these things that we as manufacturers and engi-

neers test out are intended to be used by the purchaser, and those are conditions of actual service that we have got to provide for; and until it has been ground out in the purchaser's mill we do not know just where we are at. Actual conditions of work, practice, daily use, generally in incompetent hands, determine whether the thing is going to stay on the market. I have in my experience tried to have my mechanics test a number of things and break them down, but they have had skill enough not to break the thing at the critical moment, even though they did very foolish "stunts" to accomplish it.

C. C. Palmer.—I would say in this connection that we found by experiment that no spark would ignite it under the most adverse conditions.

V. R. H. Greene.—I want to ask Mr. Palmer about the last paragraph in his paper, in which he says: "It is obvious that practically the same amount of power must be used to produce a given result, whether it is of a fixed degree, distributed uniformly over the several hours of operation, or whether it is a more intense current and used a shorter length of time and intermittently." Of course, that goes practically against the statement I tried to make last year in favor of automatic refrigeration. It is a difficult task to distribute the machine load uniformly for twenty-four hours, consequently a machine that is large enough to handle most of the day work must be installed and run twenty-four hours. That is more favorable to the central station than to the purchaser. On the other hand, if you have an intermittent running machine, then the motor is in operation only when the cooling effect is needed, and as soon as the box is cooled down the motor stops. While the starting load must be high the running load is not as great as that of a larger machine. The number of times that a machine stops and starts during the twenty-four hours more than offsets the increased cost of the starting current.

C. C. Palmer.—Do I understand you to say it is more expensive to run continuously than it would be to run intermittently?

V. R. H. Greene.—It would not be if you could reach the condition where continuous running would just about supply the needs of the box; but this condition I do not believe could be maintained.

C. C. Palmer.—Well, it cannot be regulated to such a perfect degree for a twenty-four hour run, yet we can regulate it almost to a degree where we run twelve and fourteen hours and then shut down for the balance of the night.

V. R. H. Greene.—Then your machine must be excessively large, because you are cooling the entire box contents down in fourteen hours.

C. C. Palmer.—We have to proportion our machine, of course, for the day's work, storing refrigeration during the time we are idle.

V. R. H. Greene.—That means, then, that the running load will be excessive during the time it is running. If you put in a small machine, which shall distribute the load evenly for a day, and then shut down when that temperature has been reached, you certainly save a considerable amount of power.

C. C. Palmer.—That is a question that can be determined only by actual results. It appears to me, basing my theory on the fact that we recommend continuous operation for ice making, that that was simply done as an economic proposition. It is the same in operating the small machines. If we could induce our customers to run continuously we would prefer to do it and put in a smaller machine; but we find a great many of them want to run twelve or fourteen hours a day and then lie idle the balance of the time, and to do that we run continuously fourteen hours, and then stop from sunset to sunrise again.

V. R. H. Greene.—You put in a machine just as big as you need?

C. C. Palmer.—Practically that, to do the same work in twelve hours that we would in twenty-four hours ordinarily. There is no other way out of that.

V. R. H. Greene.—If you remember, some of those blue prints that were hung up last year in connection with my paper on "Automatic Refrigeration" showed the records of a recording voltmeter. The maximum load was, of course, during the working hours of the summer months, and then toward the end of the season the machine began to shut down, and in the winter time it ran very little. The result was that the average running time figured out about eighteen hours for the summer and only six or eight hours for the winter months. It showed a big saving over anything that could be put in in the way of continuous operation, unless you keep a man there starting and stopping it, as the conditions in the refrigerator require, during the different seasons.

C. C. Palmer.—Our idea was to build a machine which required practically no care, except starting and stopping, and to eliminate anything in the way of additional expense or additional parts which would be liable to accidents.

V. R. H. Greene.—The only thing I was thinking about was whether continuous operation was really better. I can see it would be better from the lighting station standpoint to have the stopping load; but is it better for the consumer?

C. C. Palmer.—If it is better from the lighting station standpoint and better for the consumer, then we have got a friend in the lighting station.

John E. Starr.—Mr. President, some time ago it was my duty to test out an ethyl chloride refrigerating machine, which was not, however, constructed—at least as far as the compressor is concerned —in the same way as the compressor described, which is a rotary compressor. I am familiar with the compressor described and know somewhat of its use in refrigerating work, but have never tested a complete plant operating with this particular compressor. The plant that I did test was equipped with a regular reciprocating compressor, and it gave results for a small machine that were about as might be expected. In other words, as nearly as I recollect, there was no particular advantage in the working fluid as a working fluid *per se.* The performance of the machine was fair to good for a small machine. Mr. Palmer has told you of some of the advantages outside of those purely from a standpoint of the use of ethyl chloride as a working fluid, but it seems to me that ethyl chloride stands with most other working fluids, except, perhaps, if you brought it down to a very fine point and compared the power required to operate an ethyl chloride machine against some other machines, that the power might be somewhat more, on account of the fact that the machine must work under a vacuum on the suction side, and there is a little greater adverse effect from cylinder heating and valve action than there is on a compressor where higher back pressures are used. That was the general conclusion that we came to in the previous tests. Otherwise the apparatus gave fair results.

C. C. Palmer.—I would like to ask Mr. Starr what his experience is in the way of wear in these compressors after years of service, as he has had experience in that line.

John E. Starr.—In the particular apparatus that Mr. Palmer uses my experience with it was not as a refrigerating machine. It was used as an auxiliary apparatus, largely in a street pipe line work, as a vacuum pump to clear out sections of a line of ammonia. In every case it was attached to long lengths of line, and in many cases where it was put in it wore out very rapidly, not on account of any material defect in the apparatus itself, but owing to the fact that it was attached to long lines of pipe, and a good deal of dirt came back and it cut it out very badly, just as it would cut out any other quired to operate an ethyl chloride machine against some other ma-in contact with the surface of the cylinder. But where it was well

protected by a suction chamber, or what might be called a large scale trap, it operated for considerable periods of time. But the operations in any use that I put it to were intermittent—only maybe an hour or two a day or something like that, and maybe a day or two a month; so that really I cannot say anything about its continuous action.

R. L. Shipman.—There are two important facts connected with ethyl chloride. One is that the latent heat of evaporization is very low—140, something like that—and another is that the vapor tension is very low, requiring large piston displacement. A large piston displacement means that the horse power runs up enormously, caused by abnormal friction of the machine. I think that same rule will apply to this machine.

*C. C. Palmer.**—Theoretically, the power required to liquefy the gases of the several chemicals used in refrigerating work is equal, from the fact that in low-tension chemicals the pressure required is low. It is, I believe, acknowledged by all that, taking the volume of gas handled, the size of the compressor, and the weight of cylinders of equal capacity, the rotary has a decided advantage, more than enough to offset any friction factor.

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* Author's closure under the rules.

No. 53.

THE CONSTRUCTION AND ACTUAL RESULTS OBTAINED FROM AN ICE-MAKING PLANT OF MODERATE SIZE.

By Charles Dickerman, Philadelphia, Pa.

(Member of the Society.)

The actual results obtained from the operation of an ice manufacturing or refrigerating plant constructed on approved lines should be of the most value to those engaged in building or operating this class of machinery, from the fact that results obtained by special tests, or theory, as a rule, are very much at variance with the results actually obtained in regular and continued practice.

It is not with the idea of discrediting the performances of other plants, or results stated as having been obtained from the use of their special apparatus, that the following data are presented to you, but with the idea of placing before this Society the actual results and outlay obtained and required in a plant, covering a number of years, so that the average should be nearly correct.

In order that the members may be fully informed of the construction under which those results have been obtained, and at the same time to bring out all criticism possible and any expression as to the cause of not having obtained better results, I will first give a description of the construction and conditions under which the plant in question has been erected and operated.

During the fall and winter of 1902 and 1903 I, in conjunction with others, erected an ice-manufacturing plant in Philadelphia for our own use, and we were therefore allowed full scope in the choice of its construction. Some of the features were novel, while others were taken out of the scrap heaps of upward of thirty-five years ago.

With few exceptions the combination is good, although we are fully aware that it would be ridiculed by some of the fraternity as being inefficient and "away back."

We also know that different builders advertise to make from 10 to 30 pounds of ice per pound of coal, and that boiler manufacturers make boilers to evaporate from 10 to 12 pounds of water per pound of coal, but we have never had either builder or manufacturer show an actual performance covering a number of years.

However, we built this plant for the number of dollars it would earn. That, we believe, is the best measure of the value of anything

a man owns, unless ones goes into automobiles, or fast horses, which is another matter.

This plant is built on general lines shown by sketches which I will explain as I proceed.

Two vertical, single-acting compressors 13 inches in diameter by 30 inches stroke discharge through an oil separator into a double pipe ammonia condenser of six banks, 3-inch and 2-inch pipe, each twelve pipes high by 18 feet long, the water passing upward through the inner pipe, the gas downward through the annular space, through the usual ammonia receiver, and expanded into twenty-four 2-inch evaporating coils, each six pipes high, 44 feet long, a total of 6,336 feet of pipe. Feed of ammonia is divided into six sections of four coils each, down and up through each alternate coil. The return from the evaporating coils passes into a header and then through a 3-inch line to the gas compressors.

The forecoolers (for the distilling water) consist of three galvanized iron sleeves or shells 12 inches in diameter by about 16 feet long, with heads in each end, provided with stuffing boxes, fitting around the 3-inch gas return pipe, the distilled water filling the space between the sleeve and the pipe, passing from one to the other in the reverse direction to that of the gas.

The freezing tank contains 483 cans, 11½ x 22½ x 44 inches, arranged in twenty-three rows of twenty-one to the row. The tank is provided with two bulkheads, placed at one-fourth the width of the tank from each side, and fitted with two propellers driven by a small engine in such a manner that the brine is discharged down the two sides and back through the centre of the tank.

This small engine also operates a generator for lighting the plant and office.

An automatic, direct steam-driven air compressor furnishes a vertical air hoist with the necessary power for lifting and carrying the ice.

The usual ice house for storage is placed at one end of the building, capable of holding about two days' output, refrigerated by brine from the freezing tank, pumped by a single cylinder, direct-acting steam pump.

The first year we operated the plant with a 66-inch by 18-foot return tubular boiler, having forty-eight 4½-inch tubes, with a return flue over the top, connected to a 30-inch diameter stack 125 feet high, in which we burned bituminous coal.

Having experienced trouble in maintaining a full pressure of steam at all times, as well as finding it impossible to clean the boiler

as often as it should be cleaned, without shutting down to do so, we installed a second boiler the second year, 72 inches in diameter by 18 feet long, having seventy-two 4-inch tubes.

By so doing we apparently solved the boiler question, for the following reasons:

We can carry a full head of steam at all times, not having had to slow down or close down from one year to another.

We are able to let one fire burn down, ready for cleaning, while the other keeps up the pressure. We are able to burn anthracite buckwheat coal, which is not so hard on a boiler as soft coal, and evaporating half the quantity of water in each of two boilers leaves but half the amount of scale and mud, so that it is now only necessary to clean once a year, which we manage to do in the middle of winter, and the yearly expense on the two boilers, not counting the first year, does not amount to $25, outside the labor by our own men in cleaning.

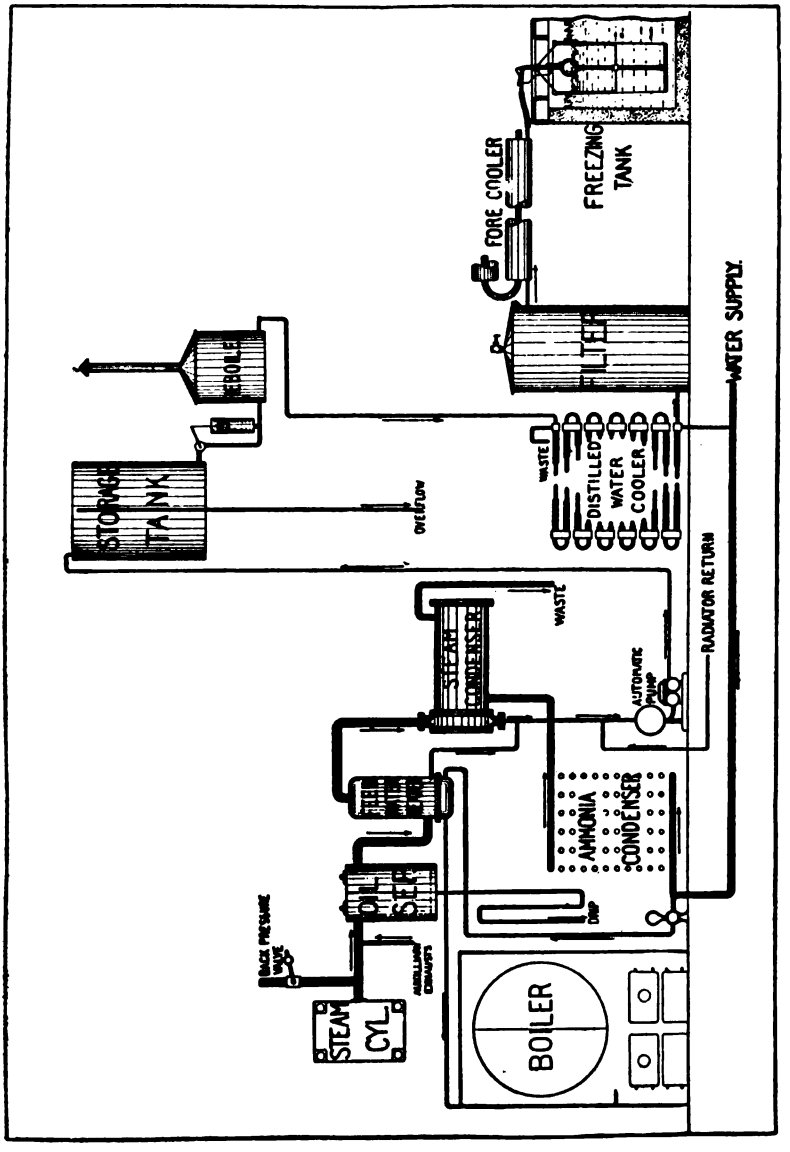
We use two boiler feed pumps, duplicate of the single-cylinder type, taking water from the city supply, discharging under pressure through a closed feed-water heater, connected up in such a manner that the condensation of exhaust steam due to the heating of the feed water is recovered and used as distilled water.

The distilled water system is somewhat out of the ordinary, and will probably be better understood by reference to the sketch which I will use in explanation.

The exhaust steam from all power parts of the plant enters a separator having a baffle plate extending down from the top head to within about 6 inches from the bottom, fitted with a siphon, which is always left open, allowing oil and drips to pass off but sealed against the loss of steam.

The first three years we kept this separator charged with coke packed in in large pieces. It gave us a little better ice during the first part of the season, but after it seemed to get charged with oil we did not think it very advantageous, and have run it the last two years empty, without any great difference in the quality of the ice. From the opposite side of the separator the steam passes to the closed feed-water heater, and thence to a steam condenser of the inclosed type, having U-shaped brass tubes.

The heater and condenser are located on a platform about 6 feet above the floor, and from each of which a pipe leads into the reservoir of an automatic, or float controlled, pump, which discharges into the hot storage tank located at sufficient height to cause the water to flow through the balance of the system. An overflow



SKETCH OF DISTILLED-WATER SYSTEM IN AN ICE PLANT.

connection is made from the side near the top, and an outlet near the bottom leading into the reboiler, the water being automatically controlled between the two by a float and butterfly valve.

From the reboiler the water passes down through two sections of 3x2 inch double pipe coolers, then upward through a charcoal filter into the cold water sleeves before referred to, and thence to the can filler and the cans.

City water supply (Schuylkill River) is connected to the bottom of each condenser section, to the two distilled water coolers, to the water jackets of compressors, and to the suction of the boiler feed pumps.

The water from the ammonia condenser passes upward through it and the steam condenser, thence to waste; while the water from the distilled water coolers and water jackets is allowed to run to waste direct. It will be observed that from the time the distilled water leaves the reboiler until it is filled into the cans it is under pressure, and that the storage of the water is while it is hot instead of cold as usual, and we are satisfied that a better quality of ice can be produced, especially under intermittent conditions, which occur during the winter months, than with the usual type of storage.

We have all overflows and drips brought to one point in the plant so that we can see the quantity and test the temperature of each, and they are more apt to be attended to by the engineer than if located in different parts of the plant.

It will be observed that this construction places the plant entirely within a one-story building, which enabled us to put up our building for less than half the cost of some other plants of a similar capacity.

The use of city water is something in which I presume the majority of you will not agree with me, but the practice has its advantages in being less destructive to the surfaces in which it comes in contact, and in not requiring to be pumped, and we have not yet decided that the advantages do not outweigh the disadvantages.

The net results of this plant for four years are as follows:

SUMMARY OF ICE PRODUCTION AND COST OF OPERATION.

	Ice, Tons.	Coal, Tons.	Wages.	Supplies.	Repairs.	Betterments and Improvements.	General Expense.
1904.....	6,667 $\frac{1800}{2000}$	1,321.08	$4,749.09	$1,266.72	$1,359.40	$1,352.02	.....
1905.....	8,720 $\frac{1400}{2000}$	1,550.16	4,677.70	936.64	823.14	756.46	$679.39
1906.....	9,144 $\frac{1500}{2000}$	1,481.15	5,398.55	837.25	1,075.36	301.15	1,614.48
1907.....	8,866 $\frac{1800}{2000}$	1,377.04	5,204.19	887.51	933.80	556.00	604.26
Total....	33,400 $\frac{1000}{2000}$	5,731.03	$20,029.53	.....	.....	.....	.....
Average.	8,350 $\frac{875}{2000}$	1,432.16	5,007.38	.....	.....	.....	.....

Adding the four columns of supplies, repairs, betterments and improvements and general expense, we have a total outlay above coal and wages of	$13,983.58
Or a yearly average of	3,495.89

Dividing the total expenditures into the total number of tons of ice manufactured, we have for the cost of ice as follows:

Supplies, repairs, betterments and improvements, and general expense	$0.42
Wages	.60
Coal	.41
	$1.43

Allowance should be made for the amount of ice produced in 1904, which you will notice is about 2,000 tons less than the general average, which was occasioned by our shutting down during January for changes in the plant which we made at that time.

Also in considering the quantity of coal, we have had steam on one or both boilers every day and night in the year.

In regard to wages, we have never laid a man off during the winter, but have turned his services into the maintenance and betterment of the plant. Also we have handled an average of 5,000 tons of natural ice a year, and we consider that the time of one man should be deducted from the above amounts, to represent properly the cost of operating the plant.

Three requisites are, in my opinion, necessary to the success of an ice-manufacturing plant: minimum cost of installation, the manufacture of the maximum quantity, and sale at the highest possible price of all you can possibly produce.

DISCUSSION.

Charles Dickerman.—I also want to add that in writing this paper, and getting together the figures of the actual amount of coal consumed in that number of years, we made one change, which has raised the temperature of the feed water something like 20°; and that is, we now take the feed water from the overflow of the steam condenser for the boiler feed pump instead of water as it enters the building. We had an object in the original installation, in bringing this cold water in, and that was that we increased the amount of condensation in the feed-water heater. But we think we can get enough without using cold water, and therefore we increase the evaporation of the boilers to that extent.

Albert Johnson.—What is the daily capacity of the plant you describe?

Charles Dickerman.—You can call it from 30 to 35 tons per day. The compressor is about 35 tons capacity, and the tank is about 30 tons, so that you might consider it anywhere from 30 to 35 tons capacity.

Albert Johnson.—How many days' run are your data based on?

Charles Dickerman.—The data represent four years' run, 1904, 1905, 1906 and 1907. The year 1903 was not complete, therefore I omitted it. The present year, 1908, is not yet complete, so I could not embody that.

Albert Johnson.—Did you run every day in the year, I mean?

Charles Dickerman.—We run every day that is necessary to supply our trade. We have very nearly made our rate capacity for 300 days of each year. We have averaged 30 tons a day, very nearly, for 300 days of each year.

L. Howard Jenks.—I would like to ask Mr. Dickerman how much distilled water the forecooler will hold; in other words, how many cans can be filled from the water contained in the forecooler before drawing the water from the filter?

Charles Dickerman.—To answer that question I will have to make a calculation. There are three sleeves, 12 inches in diameter, each about 16 feet long.

L. Howard Jenks.—That would be about four or five cans?

Charles Dickerman.—Yes, more than that. I should say ten or twelve. There is a plant in this city of about 400 tons, installed, I think, six years ago, in which the system of using a hot storage tank was applied. About two years ago an addition was made to it. The original capacity was 250 tons, and 150 was added, with a cold storage tank of the usual style; and of the two the operator preferred the cold storage tank, for the reason that during certain times of the year when they overdraw, when they overfill the cans, there is a supply of cold water to draw from, whereas with the hot storage tank there is no reserve of distilled water from which to overdraw.

Gardner T. Voorhees.—Does the water go through the sleeve in the forecooler, that is, surrounding the gas pipe?

Charles Dickerman.—Yes, the water goes through the sleeves from one to the other in the reverse direction of the gas.

Gardner T. Voorhees.—You never had any trouble with freezing up in this apparatus?

Charles Dickerman.—We have had no trouble. We do not get the water that cold.

Albert Johnson.—How many tons of ice did you get from a ton of coal?

Charles Dickerman.—I think the average is about 6½ tons of ice per ton of coal.

Albert Johnson.—What is the price of your coal?

Charles Dickerman.—As a rule we have paid from about $2.40 to $2.50. This year we are paying $2.50.

Albert Johnson.—That would make it about six to one?

Charles Dickerman.—Six and a half, yes. You mean 2,240 pounds?

Albert Johnson.—Yes.

Charles Dickerman.—Of course, you cannot consider that is what the plant is capable of manufacturing. That is what it does during twelve months of the year, or rather during forty-eight months. That includes the winter months, in which the plant is not operated for days and weeks at a time; nevertheless, the plant is kept warm and the office heated. I think that increasing the heat of our feed water from 175°, as I found it some time ago, to 205°, or possibly 208° and 210°, will make itself shown in the future coal consumption.

D. S. Jacobus.—What does the item of expense cover?

Charles Dickerman.—General expense is that which we cannot consider chargeable to any other heading. There are rent of the railroad siding and traveling expenses, carfare and things which we cannot put under the heading of wages, supplies or repairs. I also wish to call your attention to the fact that every item of expense is in the cost of this ice. Every cent that has been paid out in the plant in four years has been counted in the cost of the ice, although we have an item of $2,900, practically $3,000, for betterments and improvements. But I thought it better to put in the total outlay of money, because betterments and improvements are very often nothing but maintenance, if you come to sift them down. Very likely a great deal of the money that we have put in the plant for betterments and improvements is really nothing more than maintenance, because there are other parts of the plant that are deteriorating to that extent.

D. S. Jacobus.—Does it include the salary of officers?

Charles Dickerman.—We have no salaried officers.

D. S. Jacobus.—That is the cost of ice on the platform?

Charles Dickerman.—Yes.

Gardner T. Voorhees.—Do you have any exhaust steam coming out of the free exhaust pipe into the air?

Charles Dickerman.—No.

Gardner T. Voorhees.—None at all?

Charles Dickerman.—We are within the limit of economy of the feed-water heater and boiler, so far as the single effect is concerned.

Gardner T. Voorhees.—No live steam used?

Charles Dickerman.—Yes, we have to supply all the way from 10 to 20 per cent. of live steam more than the exhaust steam of our plant, and we at all times have live steam turned into the feed water. We apply what live steam is necessary to make up the quantity of water at that point.

Gardner T. Voorhees.—So that your total machinery and auxiliaries are more economical—that is, they do not give you enough distilled water?

Charles Dickerman.—By from 10 to 20 per cent.

Gardner T. Voorhees.—The engines and auxiliaries give from 10 to 20 per cent. too little water?

Charles Dickerman.—Less water than the machine is capable of freezing.

D. S. Jacobus.—You said you used city water. What is that used for?

Charles Dickerman.—Everything.

D. S. Jacobus.—Under what item is the cost of the water put?

Charles Dickerman.—Supplies.

Albert Johnson.—What is the temperature of that water?

Charles Dickerman.—Fifty degrees at the present time and 79° in the summer.

John C. Sparks.—Where your plant is situated in Philadelphia, do you get filtered or unfiltered city water?

Charles Dickerman.—Perforated coil. We have a horseshoe present year, but I believe we are now getting part of it filtered.

John C. Sparks.—Do you use the open or closed coil in the reboiler?

Charles Dickerman.—Perforated coil. We have a horseshoe coil perforated on its upper surface, and the steam blows into the distilled water.

John C. Sparks.—I think it is Schuylkill River water, is it not?

Charles Dickerman.—Yes.

John C. Sparks.—Do you find that it very quickly uses up your charcoal? How often do you renew the charcoal filter?

Charles Dickerman.—We renewed it every year up to last year, when we let it go for two years.

John C. Sparks.—At the end of that time what was the nature of the core of the ice cake?

Charles Dickerman.—The ice was just as perfect as it was at the time we started. Charcoal has nothing to do with the core of ice.

John C. Sparks.—It is a question of how nearly complete you eliminated the organic gases from your water. But take the Schuylkill water, you have got just as bad a water as you are likely to find.

Charles Dickerman.—Yes.

John C. Sparks.—Do you mean to say that you think a charcoal filter has no effect on the odor of your core?

Charles Dickerman.—Yes; has a little effect on the odor.

John C. Sparks.—At the end of two years, what was the effect on the odor of the core?

Charles Dickerman.—Not anything different from what it was the first six months.

John C. Sparks.—Then why did you refill the charcoal filter?

Charles Dickerman.—Because we thought it was time it was changed. We had intended to recharge at the end of the first year, but we were so busy that we could not, and so we thought it could run another year, and so it did.

John C. Sparks.—What effect did the recharging of the charcoal filter have?

Charles Dickerman.—It did some good. It made the ice a little clearer, as far as sediment was concerned, and removed some odor from the core. When you spoke about clearness I supposed you meant transparency.

John C. Sparks.—No, I did not mean that. I meant the odor of the core.

Charles Dickerman.—The deposit of particles of sediment and dirt and the odor of the core I would say would increase with the age of the charcoal.

John C. Sparks.—How good is your Schuylkill ice?

Charles Dickerman.—It is about as good ice as is made in Philadelphia. In fact there is an unusual demand for our ice. People come great distances for it, on account of its transparency. I am not talking about purity now, but of transparency, and that I attribute to the features between the reboiler and storage sleeves which I mentioned in my paper. I believe we can make a better quality of ice by storing the water hot and reboiling it at that point, and then pass it through the entire circuit under pressure, until it is in the can. I have noticed this in three or four instances, and in fact in every plant in which I have seen those features incorporated the quality

of the ice, so far as the transparency is concerned, has been exceptionally good.

A. C. Bishop.—When using the enclosed system do you experience any trouble in keeping oil out of the centre of the ice block? I know of a plant making exceptionally clear ice, but the operators have not been able to eliminate this trouble. They have a small quantity of oil in the centre of the ice, about a foot from the bottom. They have gone back to the old system.

Charles Dickerman.—We have no trouble whatever with our ice, and we consider it the best ice that is made today. We have no oil that is discoverable.

Henry Torrance, Jr.—Can you tell me how much drip there is in the separator; how much steam there is wasted there?

Charles Dickerman.—I cannot tell the percentage of waste.

Louis Block.—What is the cost of the water? You have not shown that.

Charles Dickerman.—No, the water comes in as a supply. The cost of water would amount to about three cents a ton. It amounts to exactly a dollar a day.

Louis Block.—You are not using a cooling tower?

Charles Dickerman.—We are not.

Louis Block.—The water is cheap?

Charles Dickerman.—A dollar a day. I do not know how many gallons. I never measured it.

Louis Block.—You just pay by the day?

Charles Dickerman.—No, we pay by the size of the supply pipe.

Louis Block.—Oh, no meter?

Charles Dickerman.—No.

Gardner T. Voorhees.—Mr. Dickerman, I want to ask if you have a foul gas vent from the steam condenser.

Charles Dickerman.—There is none.

Gardner T. Voorhees.—There is nothing to let the foul gas out from the condenser?

Charles Dickerman.—Nothing.

Gardner T. Voorhees.—It goes right to the pump?

Charles Dickerman.—Yes, the pump reservoir is vented. The pump reservoir has a standpipe which reaches up, I should say, about 3 or 4 feet, and that is constantly open, so that any vapor which comes down with the distilled water can pass off at that point.

Theodore Kolischer.—Do you receive the water into the plant under city pressure or do you pump from the mains?

Charles Dickerman.—Under city pressure. The water flows in by city pressure, and is piped to the different points required.

George W. Wright.—What is the capacity of the storage tank, and can you give us an idea of the temperatures of the water in the storage tank and the water going into the freezing tank?

Charles Dickerman.—We have never taken the temperatures of the water. It is discharged directly into the storage tank from the pump, and is at about the temperature that it issues from the steam condenser.

George W. Wright.—You do not know how much loss you have between your storage tank and the reboiler?

Charles Dickerman.—No, we do not know how much loss we have. All we know is we bring the temperature up to the boiling point.

George W. Wright.—With what—live steam?

Charles Dickerman.—Yes, and the only steam or vapor issuing from that plant is out of the vent pipe in the reboiler. We never have steam or vapor coming from any other place.

George W. Wright.—What is the temperature of the water going into the filter?

Charles Dickerman.—I presume the water is 85° in the summer time.

George W. Wright.—It goes to your cans at what temperature?

Charles Dickerman.—Well, I could not tell you that, any more than just by the sense of feeling. I suppose it goes somewhere about 50°—possibly as high as 60°.

John E. Starr.—Referring to the question of the cost of ice by the ton, if I understand you, this cost of $1.43 is purely and entirely what might be called shop cost; that does not include any wages of any president or secretaries or high salaried officers?

Charles Dickerman.—Yes; we have no salaried officers.

John E. Starr.—Does it include the administration? That is, I mean to say, the bookkeeping?

Charles Dickerman.—It includes the bookkeeping and clerk hire, but not any officers' salaries.

John E. Starr.—It does not include any overhead charges—interest or anything of that kind?

Charles Dickerman.—No.

John E. Starr.—It then might be called purely shop cost, plus the bookkeeping?

Charles Dickerman.—Yes.

Theodore Kolischer.—How do you sell your ice, wholesale on the platform, or do you retail it and deliver it?

Charles Dickerman.—No, simply on the platform for anybody that comes for it.

Theodore Kolischer.—Then you have no great selling expenses?

Charles Dickerman.—No, one man delivers and handles the entire output. He simply sells to the small trade, where they come for it, the scrap ice, cut up.

Gardner T. Voorhees.—I would like to ask what your suction pressure is on the ammonia coils?

Charles Dickerman.—Evaporating?

Gardner T. Voorhees.—Yes.

Charles Dickerman.—About 18 or 20 pounds.

Gardner T. Voorhees.—Your compressor pressure?

Charles Dickerman.—It depends on the season. All the way from 90 pounds in the winter to 210 pounds in the summer.

Albert Johnson.—Mr. Dickerman, it seems that the great value of this table lies in the classification of the cost of ice per ton. As far as the plant is concerned, it is just the same as we would have built twenty years ago. I cannot see anything different. The cost of ice, $1.43 a ton, seems to me high. If an ice-machine man would go to a customer, for instance, and tell him the ice would cost $1.43 and expect to sell a plant, he would not get any orders. He has to be told that it costs 60 cents or 75 cents a ton.

Charles Dickerman.—In our town we have an example of a plant in which the purchasers were told they were going to make ice for 50 cents a ton, and I think it has cost about $3.00 a ton since that time; so much so that they have had to put from $50,000 to $75,000 each year back into the plant. It is not always the story of when you are selling that comes true in experience.

Albert Johnson.—No, I guess not.

Charles Dickerman.—This is a true story.

John E. Starr.—Well, is it not true, Mr. Dickerman, in your experience, that there are plants, possibly a little larger than yours, of say 40 or 50 tons capacity, where you would expect the cost to be a little less, where the total figures taken year after year—I do not mean the figure for the maximum months, but just as you have them there, which is the only fair way to get at it, the total figures by the year—is it not true that there are a large number of plants in operation where the cost is considerably less than $1.43 a ton?

Charles Dickerman.—I do not know, of course, exactly regarding the cost of ice in other plants, but I think that ninety-nine people

out of a hundred owning ice plants are badly fooled when they come to find what ice does cost. In my cost, as I have said, there is one man's time there—well, say a two-dollar-a-day man, that would be $600 or $700 a year—which could be deducted from that annually, as this man handled natural ice, of which we sell about 5,000 tons a year. Aside from that, I believe the economy of our plant is pretty near as good as you will get. Possibly the larger plant, as you say, would be much more economical. But I believe that for one that is more economical there are ten that are more expensive. I do not believe ice is made at any price which the ice-machine people anticipate. If there is anybody who has a plant that is producing ice at a lower figure than mine, I would like very much to hear from him, and let him present figures, not covering a week or a month, but four or five years, so that we can see exactly what money it is necessary to expend in the maintenance and upkeep of the plant, and average it into the total amount of ice turned out in a certain number of years. I know that in this table every dollar we have paid out is in the expense. I also know that every dollar we have received for ice sold is in the receipts.

Albert Johnson.—I would like to ask a question. In your averages you had very smooth sailing. You say you have run almost every day. Think of the plant that is run four months in the year and is shut down the other months. What would its average cost per ton be?

Charles Dickerman.—That is just what I said, that for one that is under there are ten that are over.

Albert Johnson.—Do you not think that the cost of one ton of ice, from a 30-ton plant, with water at 50° and running twelve months in the year, is expensive?

Charles Dickerman.—The water is 50° in the winter and 79° in the summer.

Albert Johnson.—What is your ammonia bill?

Charles Dickerman.—The ammonia is in the item of supplies. Someone asked me today what our ammonia expense had been, and I could not tell him exactly, but I should say from six to eight cylinders of ammonia a year. That would be from $150 to $200.

Albert Johnson.—Do you store any ice?

Charles Dickerman.—Just from day to day. We have storage for about two days' capacity.

L. Howard Jenks.—I would like to ask Mr. Dickerman if it is not a fact that the reason calculations do not come out right in practice is that they are made on about three or four months' run? Dur-

ing the summer months ice is made at probably its minimum cost, and then after that, during the whole twelve months of the year, the apportioned cost of a ton of ice is very much higher, because it carries the losses of the winter. Your plant seems to be exceptional, because it has a long run and a flat load curve. Where the curve has a high slant I think you will find that, even with the same conditions you have shown, you will not get the same economy. In regard to what you said a moment ago, there is a plant in this city, or rather in Brooklyn, which ran for two years, making 80 tons of ice per day, the total cost of which, covering everything that was spent on the plant, was $1.05 for one year and $1 another year. It ran for two years. In that plant they delivered ice, I think, eleven months in each year. Of course that made a very flat curve, and the ice was made very economically. I think the difficulty has not been so much in the statement that you refer to, as to the estimated cost of ice figured by the ice-machine manufacturers, but is in the assumption by the purchaser that he will produce the ice for a given sum per ton on an annual basis, when he works with a calculation on a monthly basis. He finds that on a monthly basis he may make his ice for 70 or 75 cents or $1, and on the yearly basis that may be $2. The question of what his trade is to be is a question he cannot answer in advance. He usually thinks his trade is going to be nine or ten months, when, as a matter of fact, it may be only about three months. I think this is one reason why many plants fail to produce ice at what would seem to be a reasonable cost, taking the whole annual charge into consideration.

Charles Dickerman.—I think that is perfectly right, that the proposition is generally put up to the purchaser that he can make 50 tons of ice a day and it will cost so much. He does not stop to think that he will only do that a certain number of months in a year. At the same time I think the ice-machine people do not yet understand what they can do with their machines, as to what they will do. There are advertisements in the papers in which it is stated that 20 or 30 pounds of ice are produced per pound of coal—about that. I do not mean to be exact. I claim that it cannot be done.

L. Howard Jenks.—I will say to Mr. Dickerman that a plate plant, with compound condensing engines, can make 16 to 17 tons of ice per ton of coal, where the auxiliaries are run with the same steam economy, or nearly, as the main plant, and all conditions are favorable, but, of course, that is not possible with a can plant. We all admit that. I do not think a statement is due from the machine manufacturer to tell the purchaser what his trade is to be. He is not

interested in the amount of trade. He is not in a position to judge the business conditions of the plant. He is only in a position to judge the mechanical part of the plant and to state only the amount of capital, coal and labor required, and let it go at that, because he cannot get at overhead charges. In fact, I think it is a very rare thing to find the owner of an ice plant who can make such a schedule as you have there. I think it is very unusual to find an ice plant, except in the larger ones, and even then some exceptions, where the owner knows what are the subdivisions of his cost. Even in your paper you have not given us quite what we would like to know in regard to supplies and repairs, if we were running a plant. We want to compare it with our own schedule. It seems to me that if the owners of plants had a better system of keeping their accounts, so that they could subdivide their different items of charge and cost, it would be of more benefit to the purchaser and also to the machine manufacturer. In the few cases where the purchaser has the information he usually will not give it away; and that is why I am especially glad to see your paper, because it is unusual in that respect. The purchaser seems to feel that if he makes the cost of his ice known it will affect his competition.

George W. Wright.—Mr. Dickerman, there is nothing charged off there for depreciation, is there?

Charles Dickerman.—No, unless you consider the betterments and improvements, as I mentioned a while ago, as taking care of that. We carry an expense account which we have thought were betterments to the plant. At the same time I think, perhaps, they are not altogether betterments, but are more in the nature of maintenance. Therefore, I think we have kept up the plant under that heading. We consider it just as good today as it was the day we started.

George W. Wright.—You take care of your excess in the summer by natural ice?

Charles Dickerman.—Yes, that is the idea of the plant; that we run the system practically seven months in the year to full capacity and take care of the excess amount required during May, June, July, August, September and part of October by natural ice. We therefore carry this excess trade, which we take care of by natural ice in the summer, over the winter, and use our manufactured ice.

George W. Wright.—That gives you very nearly your maximum output all the year round?

Charles Dickerman.—That is exactly the reason why we get that quantity of ice in a year.

A. C. Bishop.—What do you get for ice?

Charles Dickerman.—From $1.50 to $8 per ton. In 1906 we got $8.

George W. Wright.—I went after a contract two years ago for 5,000 tons of ice, and the party who wanted it told me he had heard from six icemen in the city in which I was located that they could easily make ice for 65 cents a ton. They all told him that, and therefore there was no use of his paying $1.50 a ton when it could be made for less than $1. I told him I could not make it for that, and I am very glad to see you, for one, have an idea of about what it costs. I have heard talk about 50-cent ice, but I have never seen it yet.

Charles Dickerman.—I have heard talk all the way from 50 cents up, but I have never got any at that price yet. I think this is a low cost.

Albert Johnson.—If you sell any at $1.50 on the platform you are losing money.

Charles Dickerman.—Yes, we know it.

Albert Johnson.—If you sell at $2 you are not making anything.

Charles Dickerman.—When we sell at $2 we are not making anything, and we are losing at $1.50, and when we get $2.50 we are just barely making what we must.

No. 54.

WATERPROOFING IN REFRIGERATING WORK.

By Edward W. DeKnight, New York, N. Y.

(Non-Member of the Society.)

Waterproofing means more to refrigerating engineering than to any other branch of engineering work. To the refrigerating engineer it means not only the safety and security of a building foundation, but the perfection of that which is the very cornerstone of successful refrigeration—insulation.

What is perfect insulation is still a debatable question. We know this, however, that no insulation is perfect which is not waterproof and dry. Moist or wet insulation is not only valueless but destructive of the very purpose for which it was devised.

Insulating means waterproofing. Waterproofness means insulation. The two are correlative; they must go together—no matter in what light and for what purpose they may be considered. The asbestos lagging, for instance, enveloping and insulating a locomotive boiler, to retain the heat, would be worthless if it were not waterproofed by the steel plates covering the lagging. In the past the refrigerating engineer seems to have been amply satisfied with lining air spaces—whether filled or not—with one of the many insulating materials on the market, with not even an impervious felt, but ordinary thin waterproof paper, carelessly tacked in place. But advanced refrigeration needs more efficient and more scientific treatment.

Waterproofing applies to every part of cold storage construction—to the foundation of the building, to the walls above grade, to the roof, to the various floors, to the partitions separating the compartments, to the ceilings, etc.

The modern and correct theory in waterproofing a building foundation is to insulate it away from and out of contact with the earth. This is accomplished by applying to the exterior surface of the foundation a tough, elastic, absolutely impervious membrane or stratum, so that not only moisture but electrolysis cannot come in contact with and affect the walls, much less go through the wall.

It is absolutely impossible to accomplish this by coating the exterior surface of the walls with washes or cement plaster, which

will eventually crack; worse still, by applying a layer of cement plaster on the interior surface of the walls. This neither insulates nor waterproofs the wall, but allows its saturation clear through, with the consequent destructive action by not only moisture but that vital enemy to the embedded steel—electrolysis.

We have now reached that point where we must differentiate as between the integral or rigid system of waterproofing and the elastic or membrane method, and select that which satisfies our careful judgment.

All efforts in the waterproofing of structural work are divided into two main, totally divergent lines:

First.—Treating concrete to make it, in itself, impermeable. This may be termed the integral or rigid method.

Second.—Protecting concrete or masonry with something apart therefrom, to waterproof and insulate it. This we term the elastic or membrane method.

To sum up, the vital question is, shall water reach the concrete or not? The real point at issue, therefore, is one of method, which must be first settled before we can intelligently discuss the question of materials. On determining the method, as between the elastic and the rigid system, the production and selection of the proper materials will settle itself. We will consider the systems separately.

Treating Concrete to Make It, in Itself, Impermeable; i. e., The Rigid Method.

Treating concrete to make it, in itself, impermeable rests upon two methods:

First.—Mixing certain chemicals with the concrete to make the concrete, in itself, impermeable.

Second.—Applying a cement plaster, or wash, on the concrete to harden its surface.

The ingredients generally used are lime, silicate, soda, lye, soap, alum, etc.

One of the chief difficulties in concrete work is to obtain concrete properly mixed in the field. This difficulty, instead of being lessened, will be greatly augmented by the mixing of chemicals with the cement with the idea of making the concrete watertight. Certainly, to obtain a concrete so perfect as to be perfectly watertight will be a much more difficult thing than to obtain ordinarily sound concrete. In either case, whether with or without the incorporation of the hardening chemicals, there will always exist

zones weak in quality and density. There is also the added danger of the uncertain effect the addition of the chemicals will, in time, have upon the tenacity and the durability of the concrete itself, and especially upon the embedded steel. This is all experimental, and not tried and tested, waterproofing.

There is a fine distinction between testing concrete for strength and testing it for watertightness. The difficulty is that these two principles are confounded by those who seek to make concrete in itself watertight.

Assume, for the moment, that concrete *per se* may be made impermeable. If this impermeability will not prevent cracking, and as cracking will destroy the value of impermeability, why attempt to make concrete impermeable? Granted that even limited impermeability, as it were, is a desirable quality, is there not needed, however, something additional for dependable and perfect waterproofness for general conditions and practical work?

Waterproofness is not what it is today but what it will be years hence. Because a briquette, or cube, or box of specially treated concrete remains watertight in or out of a laboratory for one or twelve months is no warranty that concrete can be made permanently watertight for practical purposes. Assuming even that there can be had concrete in monolithic form so perfect in texture and mixture as the specially prepared laboratory sample, masses of concrete in the open are subject to conditions, especially in this latitude, impossible of ascertainment and test in a laboratory sample: to extremes of heat and cold, to settlement, to contraction and expansion, to earth tremors, both natural and artificial—resulting in fractures, the opening of pores, etc., a process which certainly does not decrease with the advance of years. Water will go through concrete in time. It will take longer to work through so-called watertight concrete, but it will eventually go through it. Otherwise it would be contrary to the law of physics and nature. The same principle applies, in greater force, to cement plaster and hardening compounds for the surface.

We have seen water drawn up, through capillarity, 15 or 20 feet by concrete. We have also seen water percolate through concrete over 20 feet thick. It may take two or three years to do so; meanwhile, the assumption is that the concrete is fairly watertight. When concrete below grade becomes damp, wet and saturated, it is almost impossible to eradicate the moisture. If the moisture freezes, expanding one-tenth its volume in so doing, it requires no stretch of imagination to calculate the effect upon the concrete or masonry.

Enough water will be taken in through a crack before the crack is filled to attack and injure the steel.

Again, many engineers believe that by increasing the steel reinforcement the cracking of concrete will be prevented and the concrete also be made watertight. We may agree that the steel strengthens the concrete, but we cannot agree that it makes it impervious. We know that steel, embedded in cement and kept dry, will indefinitely retain its purity and strength. We know also that moisture, reaching the steel, creates corrosion. The immediate effect is to destroy the bond between the steel and the concrete. The heat and expanding gas from decomposition, which is progressive, press the cement away from the steel. There then no longer exists, in fact, steel-reinforced concrete, but the very opposite, and a menace to life and property, which may eventually end in a collapse. If there is a particle of iron in the plaster on the ceiling and walls of this room it will make itself apparent by a brownish, raised spot or scale, as the result of moisture in the air attracted by and decomposing the iron.

The objections to the second method, *i. e.*, applying a cement plaster or wash to the surface of the concrete, are too numerous to mention here. It is poor judgment to depend upon but one layer of any single thing, which in this case is an inelastic cement plaster or a thin, almost imperceptible, wash, as the sole waterproof protection of any structure. This aside from any consideration of the splitting or cracking of the cement plaster. One infinitesimal pore imperfectly closed, by permitting the entrance of water, which would soon spread, would make valueless the balance of the washed surface.

Strange as it may sound, however, the advocates of the cement plaster method, while admitting the cracking of the cement from settlement, contraction, expansion, etc., recommend that it be applied to the interior surfaces of the foundation walls, for the reason that there the cracks can be seen and plastered as they occur. This seems like transferring a bad thing to a worse position.

In the first place, waterproofing must not crack; if elastic, it will not crack.

In the second place, it is against the law of nature and physics to place any kind of waterproofing in front (where in time it can be shoved or worked off) of the line of resistance (the wall proper), instead of back of the wall. To say that the material is so good and tenacious it will not be shoved off does not alter the fact that it is contrary to the laws of physics, and no amount of argument can

make it right. Being based on a wrong theory, the result must be wrong eventually. It simply requires the right conjunction of events or conditions to demonstrate the truth, though it may be long delayed.

Most seriously, however, by placing any waterproofing material, cement plaster or any other so-called waterproofing material on the interior surface of a foundation wall, one overlooks the very purpose and real theory of waterproofing, which is to keep water out of the wall. Why permit the entire saturation of the wall through and through? No one can better realize what this means than a refrigerating engineer. In the great majority of cases foundations of buildings are waterproofed, not so much because of water pressure but to prevent moisture entering the wall, working up the wall by capillarity, and destroying the steel through corrosion and electrolysis. In any case of considerable water pressure it requires no stretch of imagination to understand what would happen with a cracked waterproofing on the interior surface of a wall. Apply such practice to your own habitation. Would you desire the walls of your own home to be damp, wet, and saturated clear through, impossible to heat, and a menace to comfort and health? Would it not seem like criminal carelessness to allow such a condition solely because you placed a waterproofing on the interior surface of the wall so you could see it, when you could as readily, by the exercise of some forethought, have placed the waterproofing on the exterior and proper side of the wall, so as to make it, clear through and up, dry, warm and sanitary? If the principle be right in your own case, it will be right in any other. To say that waterproofing is visible and patchable appeals to one and sounds well, but is misleading. Suppose the waterproofing cracks under the boilers?

The great trouble is that the average engineer and architect has no time to study these important specialties, being engaged in the general proposition. He is apt, therefore, to be misled by good talking points. It is because of this and no other reason that a wrong principle sometimes is followed by even the best of men. The following of it by such men for a time gives coinage to the principle and its circulation among others. It must, however, of its own self, eventually fail.

Concrete is not an insulator and is not proof against electrolysis.

We have come to that marvelous development in our construction work where we must understand that the real present-day importance of waterproofing is not simply in keeping water out of buildings but in protecting and preserving the embedded steel. This

vital fact is entirely overlooked by *any* method of waterproofing which would place the waterproofing on the *interior* surface of a foundation wall. Not only will no interior coating of cement or other waterproofing material protect the steel, but no so-called "watertight" concrete, cement plaster or wash applied to even the exterior surface of the wall would serve, because they do not and cannot *insulate*. Waterproofing means insulation, as stated in the beginning of this paper, and the application and importance of that fact are coming more and more to be understood and appreciated by the thoughtful engineer and architect. Insulating means to separate, to get away from. There can be no waterproofing without insulation. It is a natural law. Therefore, any method of waterproofing not based on this principle is absolutely erroneous.

After due consideration, therefore, and recognizing the fact that so-called "watertight concrete" or cement plaster or washes are not in themselves insulators, does it not seem necessary and logical that we seek some other method of waterproofing than to rely upon watertightness in the concrete itself; that we get *away* from the concrete and provide something between the concrete and moisture, *i. e.*, between the concrete and the insulating material and between the concrete and the earth, to so protect and insulate it that water will not reach the concrete, whether it cracks or not? This brings us to the consideration of the second method, viz.:

Projecting Concrete with Something Apart Therefrom to Insulate and Waterproof It; i. e., The Elastic or Membrane Method.

Under this head come those materials and methods for preventing water from coming in contact with the concrete. Practically the first efforts in this direction were to coat the surface to be waterproofed with hot coal-tar pitch or asphalt, which, however, when set and cold cracked and separated with any settling or cracking of the masonry. Burlap was subsequently used to reinforce the pitch or asphalt, without, however, preventing them from cracking, and the burlap, being of itself not waterproof, did not give waterproofness. Later on there came into use for this purpose tar paper, which, however, lacks pliability and tensile strength. Tar and tar paper have been extensively used for waterproofing in the past simply because there was nothing else open to the profession. It was not until recent years that any serious effort was made to place waterproofing on a scientific basis and to make materials spe-

cially adapted to the various conditions, materials which would not become brittle, like tar and tar paper, or be injuriously acted upon by water, the salts in the earth, alkali in cement, etc. The result of this specialization has been to improve materials and methods greatly, and to open to the profession products for difficult work and special conditions considerably in advance of old-school materials.

Mastics composed of coal-tar pitch, or asphalt, mixed with sand or torpedo gravel, resembling somewhat, when finished, an asphalt pavement, are also used for waterproofing. Mastics on floors, especially where there is considerable vibration, soon separate from walls, steel columns and girders. If the mastic is made soft enough, so as not to crack in winter, it becomes too soft to bear the load of traffic in summer. The chief objection to mastics is that they crack clear through with any contraction and expansion or cracking of the masonry or concrete surface, of which they become an integral part when applied hot thereon.

Specifications also frequently require that the interior surfaces of foundation walls and floors shall be given one or two coats of some waterproofing paint. The paints might be excellent materials in themselves, but their use for such a purpose is a sheer waste of time and money, as they cannot possibly prevent, for a number of obvious reasons, the percolation of water through the wall, or protect the imbedded steel.

There are also now on the market a number of what are termed "textile" waterproofing materials, which, on examination, will be found composed, in many instances, of simply burlap, *i. e.*, ordinary commercial bagging. The fibre is vegetable, is extracted from the bark of trees, and is very perishable, especially in underground conditions. The apparent strength of such materials misleads one into using them, whereas strength alone is not, by any means, the first essential in a waterproofing material. These saturated textiles, burlaps or baggings are, in a measure, going backward to the old-school method of incorporating burlap with pitch or asphalt to reinforce it as steel reinforces concrete. There is a clear distinction, however, between the principle and results to be obtained in reinforcing concrete with steel and reinforcing waterproofing with burlapped textiles. The two should not be confounded. Otherwise it would be advisable to reinforce the bitumen with copper mesh. The treated or saturated burlap is no more waterproof, especially for water-pressure work, than when originally used to hold pitch or asphalt on a wall. This can be easily tested by placing a single sheet

or thickness of the treated burlap under the slightest water pressure (as under a spigot), when it will be found that water easily passes through the interstices of the material. A woven fabric has never proved superior for waterproofing, even though it be canvas, because the fibres pull against instead of with each other, resulting in the opening of the interstices and the usual splitting of the fabric.

The best material is unquestionably a strong, fibrous felt, made in itself, *i. e.,* in one sheet, absolutely impervious to air and water by a process of saturation and then coating with materials specially adapted to withstand the injurious action of water, and particularly all underground conditions. It is then practically an impervious membrane or skin through which, in one sheet, water will not pass. As many layers thereof as the conditions require can be then cemented together with a waterproof bitumen-cement, not too weak or hard and brittle for the felt, but as strong and elastic as the felt. This forms a waterproof stratum so thick, strong, tough and pliable that, without injury, it can be readily pulled, bent, turned, twisted, etc. Whether in a building foundation, covering the floor of a bridge or enveloping a tunnel, it readily conforms to the final conformation of the surface waterproofed, from which it is practically apart, and which it *insulates* and protects under all conditions, settlement, jars, shocks, cracks, expansion, contraction, heat, snow, ice, water, etc.

Some time ago I termed this "the membrane method," and firmly believe it to be the basis for the development of a perfect waterproofing. It is not, therefore, primarily a question of material but of method.

It is called the membrane method because it is the teaching of natural law. There is nothing made by man which has not its prototype in some form somewhere in nature. No man ever invented a color. No man ever devised an insulation for the most intricate electrical machinery as perfect as the insulation of the human brain —the dynamo of the universe. In seeking a guide, therefore, in our present problem, we find throughout nature no waterproofing which is hard or vitreous or rigid, because nature waterproofs only living things, things that move, not dead ones or inorganic ones, which do not require it, but, by moisture, heat and decomposition are resolved back into carbonate of lime. Therefore, all things that live and move require, and are by necessity protected with, a flexible, elastic skin, yielding to growth, movement, action. Therein lies the origin, the first principle of waterproofing, natural or artificial. Can any other principle be right?

In the very beginning of generation nature begins to cover, *insulate* and protect, with an elastic film, skin or membrane, the life germ. This law prevails through the whole line of plant and animal life, from a grain of mustard up to a mastodon. Puncture this protecting skin or membrane and there immediately ensues decomposition (or corrosion) in the exposed flesh. So long as the plant or animal lives, whether one or a hundred years, this yielding membrane protects perfectly. We ourselves take the tough hide and the fine elastic skin of animals to protect our feet and waterproof our hands, both our own and the artificial protection readily yielding to every move of the foot or hand.

If a chicken came forth in a coating of soap and alum or cement plaster, its usefulness would end with its appearance. Nor do we waterproof our feet or our hands by immersing them in a bath of cement, which would make them set, rigid and useless. Yet is this not essentially what we do when we would protect and waterproof our walls and structures, which must settle, contract, expand and thus move, with a coating of cement plaster or an injection of hardening fluid to embalm them, thus thwarting natural law and preventing instead of providing for the natural functions of the masonry or concrete, and also imperiling both the waterproofness and the usefulness of the structure? Obviously, therefore, a natural waterproofing is one which—skin, hide or membrane-like—yields to the natural contraction and expansion of the structure and protects it by preventing water from *reaching* it. If, therefore, the skin or membrane theory is logical, natural and right, it then simply remains for the chemist and the manufacturer scientifically to perfect materials necessary for the practical development and application of that method.

Coming down to actual work under such a method, I would submit the following:

Recommendations for Practice.

1. Properly *design* the thing to be waterproofed. Use no set specification. Each condition has its characteristics and should be carefully considered, especially with reference to the character and use of the structure.

2. Select the best materials obtainable for the desired purpose.

3. See to the correct application of the materials. In water-pressure work, as in foundations, employ only skilled labor, thoroughly experienced in such work.

FOUNDATIONS.

The walls should be waterproofed and insulated on the exterior surface with a thick, tough, elastic stratum of waterproofing composed of alternate layers of impervious felt and bitumen. Use as many layers of felt as the conditions require, according to the water pressure, etc.; in any event, use not fewer than two layers of felt. Carry the stratum under the foundation floor. Thus waterproof and insulate the foundation, forming practically a waterproof box in which the foundation proper is set.

It pays to protect the waterproofing stratum on the walls with a layer of cement mortar or a course of brick.

WALLS ABOVE GRADE.

In the perfect cold storage structure there can be well afforded above grade an outer or curtain wall and an inner or wall proper. Line the interior surface of the curtain wall, or if no curtain wall be used the interior surface of the wall proper, with one layer (two would be better) of impervious felt cemented to the wall, thus forming thereon a membrane through which neither air nor moisture can pass into the air space or insulation.

Similarly treat all subdivisions of the wall which may be divided into air spaces, whether or not they be filled with an insulating material. This forms on both sides of the insulating medium an impervious, airtight membrane, which not only perfects the air space but preserves the efficiency of the insulating material by protecting it against the entrance of air and moisture.

Also similarly treat the floors, ceilings and partitions of each compartment. When the compartments are of wood, lap the felt sheets 3 inches, cement the laps only, and tack them.

FLOORS.

Under and over each layer of insulating material apply one sheet of impervious felt, lapped 3 inches, and the lap tightly cemented.

If the wearing or walking surface of the floors be of mastic or of concrete, apply under same three layers (though two would serve) of impervious felt, thoroughly cemented together with the special hot bitumen, so as to form a strong, pliable waterproof stratum under the wearing surface. This will effectually stop any

moisture and water from condensation or other causes working its way down through cracks in the cement wearing surface and destroying the usefulness of the insulating material.

Cracks, in greater or less degree, eventually develop in all cement or mastic (asphalt) wearing surfaces, especially under refrigeration conditions. It will be admitted that it is not only the large quantity of water which settles on or flows over a floor, and by evaporation or drainage disappears, with which the refrigerating engineer is most concerned, but the small quantity of moisture or water which, through even the smallest crack, works its way through into the insulation and immediately turns it from a medium of value into one of absolute destruction. These small, apparently harmless cracks—more often unseen than seen—are like the small worm which sinks the ship. Against these cracks and defects the membrane method at all times, whether the temperature be up or the temperature be down, affords a perfect protection to the underlying insulation. It is absolutely impossible to permanently obtain such protection by any coating, or wash, or layer of cement plaster, applied to the surface of the concrete floor.

Roof.

Cover the roof with three or four layers of impervious felt cemented together with hot bitumen. Cover this waterproof stratum with a course of brick laid flat or a thin layer of cement, over which place good soil 6 inches deep. Then sow this soil with grass seed. This will provide a cool, insulating roof which cannot be excelled for cold storage buildings. At the same time it utilizes valuable roof space which now goes to waste.

Finally, in refrigerating construction we must above all else keep moisture out of the insulation—in other words, "keep our powder dry."

The theory that waterproofness means insulation must be so studied and perfected by the refrigerating engineer as properly to include the entire structure—foundation, roof, walls, interiors—and not until then will his work be crowned with the highest success.

DISCUSSION.

John E. Starr.—Mr. DeKnight states that a membranous covering is used to keep out the moisture, and I ask, assuming that a por-

tion of the membrane is punctured, what the result would be. It would seem to me that, depending somewhat on the extent of the puncture, the moisture would spread through this puncture and invade the concrete that it was intended to protect. I would like to ask the reader of the paper what precautions or what reason he has to advance that in the ordinary course of construction punctures will not be made in this membranous covering. What are the safeguards which are put around it? It must be admitted, of course, that if a small hole is made in the membrane and water starts to pass through it will do so to a greater or less extent between the membrane and the structure to be protected, and granting that the interior of the structure is not in itself impervious, there might be danger of moisture passing through. I have no doubt that Mr. DeKnight can answer this, not exactly an objection, but a question.

Edward W. DeKnight.—I think Mr. Starr has hit upon one of the most important features in all waterproofing work, no matter what the waterproofing may be, and that is the perfection of application. It seems to me that the average architect or engineer pays very little attention to the protection of his waterproofing when it is being applied. He can have no more important clause in his specification than one providing for proper protection of the waterproofing. The membrane method when applied to a floor should be applied under certain conditions; that is, workmen must not throw axes, tools, brick or débris on it; the specifications should require that there be always on the work a competent inspector. As a rule, waterproofing is done under conditions in which it is almost impossible to get it applied properly. There is usually not sufficient head room, and three or four trades working together do not give the waterproofing workmen proper facilities. As to protecting the completed waterproofing, my paper stated that on walls the waterproofing should be protected after it is in place by, preferably, a layer of brick up against it. The same thing should be followed on the floor immediately the waterproofing is in place; that is, it should be covered with an inch thick layer of cement mortar, boards or brick, according to the conditions.

John E. Starr.—In other words then, if I understand it, the question is largely one of personal equation, or the most careful inspection. If I understand Mr. DeKnight aright, it is that the whole thing, the whole success or failure of the construction which he proposes, depends on the effect of what might be called the personal equation, or rather the infinite inspection and care with which

the work is done, and on that alone would rest the integrity of the entire structure.

John C. Sparks.—I would like to ask Mr. DeKnight a question. I think that the ordinary waterproofing compounds do not entirely waterproof. I have tried many varieties. But there are some concrete tanks being used now for chemicals, for high acid solutions, which are quite waterproof. Whether they would stand low temperatures or not I do not know, but they will stand high temperatures. This waterproofing compound is silicate of soda put on with asbestos and then washed with muriatic acid, and that will stand acid all right. But the ordinary so-called waterproofing compounds reduce the water absorbed by concrete blocks from 23 per cent. of moisture, before being waterproofed, to about 6 per cent. In regard to the electrolysis that is taking place on the steel reinforcing, I think that is an assumption, is it not? No concrete walls were ever torn down to see what the effect was. There would be a slight oxidizing action on the steel, but why should it be electrolysis? It does not follow because moisture gets into steel that the action would be electrolysis. Does that follow?

Edward W. DeKnight.—Electrolysis and corrosion are practically the same thing.

John C. Sparks.—Not at all.

Edward W. DeKnight.—I mean corrosion has the same effect as electrolysis. The air is vibrant with electrical energy. The earth is. Twenty years ago our conditions were not as they are now. The engineer and the architect must protect our structures in different ways than was done twenty years ago, because everything is vibrant with electrical energy. The last annual meeting of the American Institute of Electrical Engineers paid particular attention to the growing danger to our skyscrapers and other buildings from electrolysis. I would refer the gentleman to a paper read before that society. The paper was so interesting that the New York *Herald* last summer, as I remember, devoted an entire page to it, with illustrations showing the corrosion and destruction of structural steel from electrolysis in New York city and elsewhere.

John C. Sparks.—Not concrete?

Edward W. DeKnight.—Not concrete, but it is an acknowledged fact that electrolysis will go through concrete 5 or 10 feet thick, and that concrete is not an insulator. We know that moisture is the best conductor of heat and cold, and that if we touch electrically charged water passing through the air, as from a hose, if the volt be strong enough it will kill us. Now, water electrically will follow the

minutest particles of moisture through 10 feet of concrete and affect the imbedded steel.

Theodore Kolischer.—Mr. President, I have carefully listened to the able paper of Mr. DeKnight, also to the simile between waterproofing and insulation in comparison with the human skin. I never have heard that skin was solid or absolutely waterproof or insulation perfect. I believe—if I am not mistaken—if you were to insulate the human body completely, living man would die in five minutes. The skin is composed of millions and millions of pores. You could not call that insulation. I mention this because the skin simile has been taken as a strong argument and a comparison. I would like to hear from Mr. DeKnight how he arrived at this conclusion.

Edward DeKnight.—The gentleman has referred to insulation. I have two meanings for the idea in the paper. Possibly I did not make it appear in the sense I desired. Insulation may be against electricity, water, heat or cold. I think we will all agree that there is nothing in nature which we do not pattern. I tried to illustrate by referring to the brain. There is no artificial thing in the world the insulation of which is so perfect as the human brain. Yet if we put a volt on the skin of the skull—not the hair—it will kill one. The pores in the skin make it non-electrically insulation-proof. I do not mean non-waterproof. If the human brain were not so perfectly protected and insulated we would die immediately. Unfortunately the paucity of hair on my head does not mean much insulation there. However, we rob our animals of their hair, after they are dead, at least, and with it insulate our refrigerating cars and the walls of our cold storage buildings. Whether it be the proper practice or not I will not say. But I am very glad the gentleman brought up the point, because after the many discussions which I have had before engineering societies I want to say this—this is the first time that point has ever been mentioned. The engineer, as a rule, is a practical man. He wants to get at how to do a thing as quickly as possible, and he has not time, though he may have the disposition, to go into theoretical details. We will never, however, arrive at any correct method of waterproofing unless we first get down to the right principle. For instance, if we believe that the membrane method is right, and that we should keep the water away from and out of contact with the concrete or thing itself, then we should develop that method. If, however, we believe that the concrete itself can be made watertight, and it is an insulator, and water will not go through it, even though it crack, then we should develop the concrete method and give up the other method.

John C. Sparks.—May I ask Mr. DeKnight why he calls this a membrane at all? A membrane is really a material through which liquids of different densities and specific gravities and behavior pass at different speeds, according to their properties. If you have a diaphragm with two different liquids you can separate the two different liquids by a membrane by the speed at which they go through the membrane.

D. S. Jacobus.—I would like to ask how long the covering has been in use. Are you sure that the hair felt will not disintegrate in the way that it sometimes does when used for a pipe covering?

Edward W. DeKnight.—It is a good question, and I can only say hair felt has been in use many years, and much longer than any other method of which I know.

Theodore Kolischer.—I also notice, with your permission, recommendations for practice: "Properly design the thing to be waterproofed. Use no set specification. Each condition has its characteristics and should be carefully considered, especially with reference to the character and use of the structure." Considered by whom? Have no set specification? Then who shall select the best material? I would like to hear from you on that. If there is no set specification who shall select the best material?

Edward W. DeKnight.—I do not know if I correctly caught the gentleman's point, but if he knew of the millions of dollars' worth of work in fine buildings and structures almost ruined or seriously injured by water from the imperfect designing of structures to receive waterproofing properly, he would not be surprised at the statement contained in the paper. When I said accept no set specification I meant this: Take a case in our own experience, for instance; we have drawn many specifications for engineers for the foundations of buildings. Someone got hold of one of these foundation specifications and used it for a reservoir. The result was that in time the water practically disappeared from the reservoir. A specification or set specification for a building foundation would never do for a reservoir. A set specification for a roof would practically never do for a railroad bridge floor. The engineer must study the use for which his structure is intended, and the character of it, and the location of it—whether it be in the Philippines or in this country. For instance, in the Philippines you certainly would not put on a roof the kind of material you would use in this country. In cold storage work the greatest mistake is in putting in a cold storage building the same kind of waterproofing material that is put in any other building. The same quality of waterproofing materials

used in a floor of a warehouse or factory should never go in the floor of a cold storage building.

Theodore Kolischer.—Well, pardon me. I feel that we have not been enlightened, or else I have not been, or Mr. DeKnight did not properly understand the point. "Properly design the thing to be waterproofed. Use no set specification." Then what is the engineer, or whoever is in charge, to do? "Each condition has its characteristics and should be carefully considered, especially with reference to the character and use of the structure. Select the best materials obtainable for the desired purpose." There is no specification, yet the best materials should be selected. Who is going to select them? I thoroughly appreciate what Mr. DeKnight says about the qualifications of different materials for different localities and all that, but it does not answer just exactly the point in the question. Now, if we engineers or architects, say constructing engineers, are supposed to have charge of a structure and to submit no set specification, yet the best materials are to be selected, I would like to be enlightened on that point for the interests of this Society.

Henry Torrance, Jr.—As I understand it, Mr. President, you engage some consulting engineer and pay him his price to tell you.

Edward W. DeKnight.—Mr. President, I was waiting for some member to say that. I did not have the courage, as a non-member, to say it.

John C. Sparks.—I would like to say, Mr. President, in regard to the remark Mr. DeKnight made about waterproofing reservoirs, that in connection with the Water Department of the City of New York we have had a concrete tank up at Jerome Park twenty-two months with no waterproofing whatever, and no leak in the concrete, except one seam where the work had stopped and then went on again. That has been in use for eighteen months, through summer and through winter, and no leaks showed at all. We have had it without any inlet or outlet for a period of three months at a time by actual measurement. That concrete tank, I know, has no waterproofing at all.

No. 55.

STANDARD METHOD OF TESTING REFRIGERATING MACHINES.

By D. S. Jacobus, New York, N. Y.

(Member of the Society.)

The subject of establishing a standard tonnage basis for refrigeration and of preparing a code of rules for conducting tests of refrigerating machines has received much attention from the American Society of Mechanical Engineers. A committee was appointed in December, 1903, to suggest a standard tonnage basis for refrigeration, and the same committee was reappointed in 1904 in order that it might prepare a code of rules for conducting tests. The committee rendered a preliminary report on the first part of the work, which was accepted by the Society in 1904. A preliminary report has been made on the second part of the work, which was presented at the Indianapolis meeting in May, 1907, but it has not been offered for acceptance, and consequently no action has been taken regarding it.

In view of the fact that The American Society of Refrigerating Engineers should be especially interested in this work it has been deemed advisable to present what has been done by our sister society for discussion, with the recommendation that a committee be appointed to act jointly with the committee of the American Society of Mechanical Engineers in preparing a report. Should such a committee be appointed, it would not necessarily follow that the report which has already been accepted by the American Society of Mechanical Engineers to establish units of refrigeration would be binding on the work of the joint committee. The members of the joint committee could, therefore, start at the very beginning if they so wish, establish the units to be used in the various measurements involved, and prepare a code of rules for conducting tests. On the other hand, they could accept the report which was presented to the American Society of Mechanical Engineers on the units of refrigeration, which is more in the line of definitions than anything else, and start their work from this point.

What I would like very much to accomplish would be to have this important work conducted by a joint committee of the two

societies, and co-operated in by all the members of each society who may be interested in the subject. Reprints have been made of the work already done by the American Society of Mechanical Engineers, and it is earnestly hoped that these will receive free discussion at this meeting. What assists a committee more than anything else is honest criticism of its work, and it is hoped that no one will hold back with the idea that he might give offense by expressing his opinion. The only way of preparing reports of this kind that shall have a maximum value is to go over them critically and weed out the weak portions, and replace them by something better, and the members of this Society can rest assured that they can give no offense no matter how general this weeding out process may be.

On examining the reprints of the work done by the American Society of Mechanical Engineers it will be seen that the unit recommended by the committee to measure the cooling effect, or the refrigeration, is one which through its long use is already a standard, viz., the heat required to melt one pound of ice. This was taken at 144 B. T. U., and on this basis the unit for a ton of ice-melting capacity is 288,000 B. T. U. The usual value employed for the heat to melt one pound of ice is 142.2, but the committee deemed it advisable to use the figure determined by Bunsen, which is 144 B. T. U.

It was thought advisable to establish a set of conditions under which refrigerating machines might ordinarily be tested, the idea of doing this being that if such a system were adopted it would be possible to compare directly the results secured with one machine with those secured by another. The standard set of conditions recommended by the committee to measure the cooling effect, or the the point of liquefaction in the condenser shall be 90° Fahr. and the temperature of evaporation of the liquid in the refrigerator 0° Fahr. For an ammonia machine this would be equivalent to saying that the pressure in the condenser shall be 168 pounds per square inch above the atmosphere, and 15 pounds per square inch for the pressure in the refrigerator. The capacity of the machine determined under these conditions was termed "the commercial tonnage capacity," the idea being that this would be a good set of conditions on which to base the commercial rating of a machine. In computing the commercial tonnage capacity, the actual weight of the refrigerating fluid circulated between the condenser and the refrigerator, and actually evaporated in the refrigerator, is employed as a basis. The refrigerating capacity obtained in this way should correspond very closely with the actual refrigerating capacity of a machine as determined by the quantity and range of temperature of the brine or other

substances cooled in the refrigerator. It will be noted on examining the code of rules for conducting tests of refrigerating machines proposed by the committee of the American Society of Mechanical Engineers that only machines employing ammonia as the refrigerating agent are considered, although the rules apply to any other machines using a condensable gas or vapor as the primary agent. It may also be noted that the refrigerating capacity of a machine is considered as referring only to that part of the plant in which the ammonia or other primary refrigerating fluid is circulated, and that the capacity of that part of the plant where cold brine or other secondary fluid is made use of for refrigeration is not considered. A test made with a brine heater would give correctly the capacities specified in the report. The standard set of conditions specified by the committee are those which often exist in ice making, and it was thought that this was a fair set under which the results secured by various machines might be compared.

An account of the work done by the American Society of Mechanical Engineers was presented by me to the First International Congress of the Refrigerating Industries held in Paris, with the request that it receive free discussion. Such discussions will be available for the use of the joint committee. In presenting the work of the American Society of Mechanical Engineers to the International Congress the following questions were asked in order to bring out discussion:

(*a*) Is it well to establish a standard set of conditions under which refrigerating machines should be tested, in order that the results obtained for one machine may be compared with those obtained with another?

(*b*) Is the standard set of conditions which have been set forth the most desirable to adopt, or could others be employed to advantage?

(*c*) Is it well to recommend that the tonnage capacity be based on the actual weight of refrigerating fluid circulated between the condenser and the refrigerator and actually evaporated in the refrigerator, or would it be better to rely in all cases on the determination of the actual tonnage capacity generated by the machine as given by the actual weight of brine or other liquid refrigerated and the range of temperature?

(*d*) Should the refrigerating capacity of a machine refer only to that part of the plant which has the ammonia or other primary refrigerating fluid in circulation, or should it include the entire plant,

and in this way be affected by the capacity of the refrigerator or the refrigerating coils?

(*e*) Is the method proposed in the report for weighing the amount of anhydrous ammonia a good one, or would it be preferable to obtain the weight in some other way?

(*f*) What is the best method of determining the density of liquid in an absorption machine? Where the liquid is very rich in ammonia it is impossible to draw it off at ordinary temperatures without allowing considerable of the ammonia to escape in the form of fumes. Would it be a good plan to pass the liquid through a coil placed in a freezing mixture and determine the density at a much lower temperature than that usually employed, and if so, what temperature would be the best one to adopt?

(*g*) What is the best method of determining the purity of the anhydrous ammonia in a machine? In case the amount of moisture is to be determined, can this be done accurately by means of absorption tubes?

(*h*) In case water is found to be present in the ammonia, how can this be allowed for in computing the tonnage capacity on the basis of weight of the refrigerating fluid circulated?

(*i*) Would it be a good plan to rate refrigerating machines in regard to capacity in some way irrespective of results which may be obtained by tests? For example, would it be advisable to rate ammonia compression machines on the size of the ammonia compression cylinders, or the displacement of the piston of the ammonia cylinder; or, on the other hand, should the rating of machines be left entirely to the manufacturers?

In addition to these general questions there are a number of other important points that should be carefully considered, and I earnestly hope that the matter will be gone into by this Society in a thorough way with a view of establishing a report which will be of value. If a report is not actually used in connection with the work which it outlines it is certainly a failure. The members of this Society have use for a report on the subject at hand, and are better able than any other body of men to prepare it in the right way. Let us all pull together and see what we can do in this line in the light of the very latest experience, and establish something which we can conscientiously feel will be of service to the profession at large.

No. 56.

REINFORCED CONCRETE FREEZING-TANKS.

By William M. Torrance, New York, N. Y.

(*Non-Member of the Society.*)

Concrete, properly made, is an ideal substance to use for freezing-tanks. The materials necessary are quickly and easily assembled at the site at nearly any location, and work can be started on the construction at once and the tank made ready for piping in minimum time. In the case of the use of steel tanks time must be allowed to assemble the parts at an ironworks shop, after which these parts must usually be shipped a considerable distance by rail, with customary freight delays, thus delaying the installation at the plant and its completion for starting business.

Reinforced concrete engineers now have the subject of stresses and strains in their specialty as well in hand as is the case in the design of steel work, and reinforcing steel is easily placed in the concrete base and walls of the tank where needed to keep the structure from cracking from hydraulic or temperature stresses. Competent concrete constructors may be found who know how to make and place the concrete properly, so as to make it water-tight and avoid planes of cleavage or of weakness sometimes found in the work of less competent contractors. The forms must be designed and the work laid out so that there will be no stoppage of work, no places where new work has to be joined to old work, but the work must all be done for any one tank so as to make it truly monolithic.

The concrete tanks with which I have had experience were made as shown in Fig. 1. The reinforcement was cold twisted steel bars five-eighth inch square placed 12 inches, centre to centre, in each direction, so that the entire area of base and sides was gridironed with these bars. By calculation for range of temperature of from — 10° Fahr. to + 90° Fahr. these rods were found sufficient to take care of temperature strains. The vertical rods in the side were made continuous with those crossing each other in the base, the bend at the corner being made cold so as to preserve for the steel the extra strength due to twisting the rod in the first place. This bend, it will be observed, is near the inner surface of the concrete, while the gen-

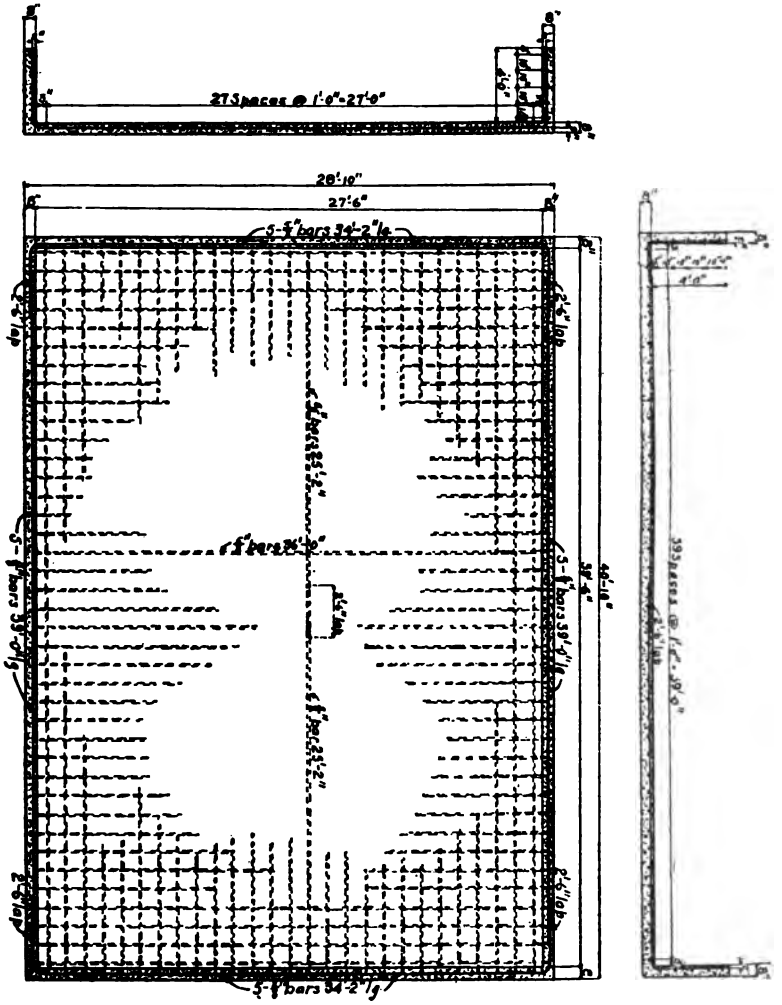


FIGURE 1.—CONSTRUCTION OF REINFORCED CONCRETE FREEZING-TANK.

eral location of rod is near the centre of the slab. This is brought near the inner surface to take care of the stresses induced at the corner by the hydraulic pressure of the brine against the sides of the tank.

In these tanks the insulation consisted of screened cinders filled with coal-tar pitch. This kind of insulation is not recommended, I am well aware, by some members of your profession, but nevertheless has much to recommend it. Cinders cost nothing but transportation as a rule—very little more than that, in any case—and coal-tar pitch, besides being comparatively inexpensive, is impervious to moisture as well as being a fairly good insulator. There can be little question that, being a mineral substance, its life as an insulator will exceed that of a purely vegetable substance. Being incapable of absorbing moisture it will always remain insulating. If the cinders are well screened of all dust and the spaces between the individual pieces of cinders deposited loosely are thoroughly filled with the boiling hot pitch, one could not ask for a better result than will be obtained, provided, of course, that sufficient thickness of the insulation be used. Another great advantage of this type of insulation, whether used outside a steel tank or a concrete tank, is its additional property of non-absorption of moisture, being, if well placed, a waterproofing. Thus, if this form of insulation were in use, if the brine, or any portion of it, could find an opening through the steel or concrete, it could not leak through or into the insulation, thus being wasted, and spoiling the insulation at the same time.

The insulation shown was placed on the top of the foundation before the tank was built. That on the sides was placed afterward, the outside concrete forms being moved back and the cinders and tar being placed in the space between them and the finished concrete wall.

After consultation with such authorities as were obtainable, it was decided that the concrete itself has about the same insulation coefficient as a brick wall or about half that of the cinders and tar, so that for the same degree of perfection in insulation several inches of the thickness of this cinders and tar mixture were saved, or the insulation value was increased to that extent, whichever way you look at it, by use of the concrete tank instead of a steel one.

We hear frequently of steel tanks being worn out after only a few seasons of use. This is often due, as you all know, to the habit of the operatives of strengthening their brine by adding common salt ($NaCl$) instead of the calcium chloride ($CaCl_2$) which they should have on hand, but which is either lacking or, from the opera-

tives' standpoint, too expensive. This common salt, of course, rusts the tank out very rapidly. While to many this might not be considered a recommendation for concrete, this practice would not injure a concrete tank, as neither NaCl brine or $CaCl_2$ brine would have any chemical effect on well-made concrete. In fact, with concrete tank installation it might be found to be cheaper to use NaCl altogether and not get the more expensive $CaCl_2$.

For the benefit of those who would not use anything but cork board or similar insulation, I have devised a tank of reinforced concrete with this insulation. Of course, in the use of such a substance not perfectly impervious to moisture we must protect it on both sides. It would not do to place simply 2 or 3 inches of cork board in place

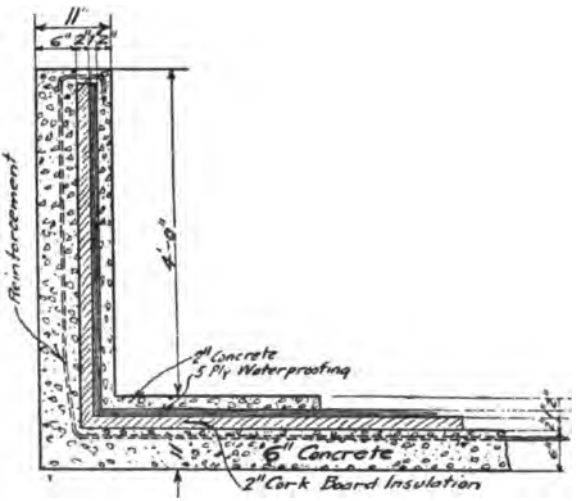


FIGURE 2.—INSULATION FOR CONCRETE FREEZING-TANK.

of the 8 inches of tar and cinders shown in Fig. 1 and let it go at that, but means must be employed to keep it from becoming saturated with moisture from the outside.

Fig. 2 shows my method of using cork insulation. This shows the tank proper, built outside the insulation, the reinforcement being of the same type as that used in Fig. 1. This reinforcement need not be so heavy, however, as it will not be subjected to the same temperature conditions as is the case in the other method.

In construction this tank would be built perfectly monolithic, as in the other case, the reinforcement being made long enough at the top to be bent over the top of the insulation and down in front, as shown. On the inside of this wall so built a coating of hot tar

would be placed on the inside surface immediately after the forms are removed, and against this a 2-inch layer of cork board would be secured by fastening it with wire or other appliances previously partially embedded in the concrete. Then inside this cork board a five-ply felt and tar waterproofing, as per "Barrett Specification" for roofing, or equally effective waterproofing, would be placed. This, as shown, would consist of six layers of tar alternating with the five layers of felt, and would be about an inch thick, adding considerably to the insulation. After this is placed and the ends of the reinforcement rods are bent over and down as shown, a protecting layer of concrete 2 inches thick would be placed. Light chicken wire reinforcement in the middle of this 2 inches of concrete will insure perpetual absence of cracks in it.

Complicated as this method seems, it is comparatively non-expensive, when compared with a steel tank of five-sixteenth inch metal with 4 inches of cork board outside of it. It is incomparably more impervious to leakage than this last mentioned construction, as it will not leak at all, and while the two methods start out about even as to insulation, in the case of the special concrete construction the insulation is absolutely protected against ever absorbing moisture and thus being spoiled, while in the other case, unless additional expense for drainage or protection be adopted, the insulation would in a short time become saturated and spoiled either from-leakage from within or from ground water from without. Then, of course, this construction has the same advantage in allowing the use of NaCl brine, previously discussed, as well as being absolutely waterproof.

DISCUSSION.

Junius H. Stone.—Mr. President, I would like to ask the gentleman what is the transmission of heat through the 8 inches of cinders and pitch. He says it is fairly good insulation.

William M. Torrance.—I will have to admit that I am not very well posted on that subject. The only answer I will make is concerning tanks I know of having been constructed. In these plants the work that they get out of the tanks is very good, and is said to be better than that of tanks known to the owners of those that I speak of which are made of steel. They say they get more tons of ice for the same amount of coal burned than they used to get out of the other kind of construction. That is the only way I can answer it.

Junius H. Stone.—As a matter of information I will say that the transmission of heat through that construction is approximately

7.5 heat units per square foot of surface for twenty-four hours, and through 4 inches of cork board it is 1.65 heat units.

Thomas Shipley.—I want to say that I am interested in ice tanks. I have been making an investigation of tanks used for ice making and other purposes in connection with refrigerating and ice-making machinery, and I find more trouble with concrete tanks than with iron tanks. I was down to Rocky Mount, N. C., recently to see the plant which was said to be a "world beater." I found that the plant had a concrete tank for ice making, but the tank had given out and was abandoned after less than a year's service. I went over to the plant of the Merchants' Refrigerating Company in Jersey City last week and found there a series of ice-making tanks that are made of concrete. These tanks are in very bad shape. These are only a few of the many failures of concrete tanks that have come to my notice. I do not know where you will find wrought-iron tanks going to pieces like concrete tanks do. I do not believe the claim that the use of salt brine causes excessive rusting of iron tanks has any foundation; at least it has not so far as my experience goes. With all due respect to the advocates of calcium brine, I believe that chloride of calcium brine will eat an iron tank faster than salt brine. We have had to replace a pump twice in five years because it was eaten up by chloride of calcium, and we are beginning to get a great many repairs for plants that use that brine. I do not believe that chloride of calcium is the preservative of iron that its advocates would like to make us believe it is.

I want to say another thing about freezing-tanks. The man who neglects the insulation of his freezing-tank is neglecting one of the hearts of his plant. The man who puts cinders under his freezing-tank must face a loss sooner or later. Just at present there is a large plant of our make in St. Louis where they used cinders under the tanks. It is a 225-ton plant having four tanks, and the owners are at work removing two of the tanks because the cold has penetrated the cinders and frozen the ground underneath to such an extent that it has lifted the centre tank so much out of line in the middle that the only thing to be done is to thaw out the ground under them and put in proper insulation. This cinder insulation was put in because it was cheaper than cork or other good insulation, but the sequel shows the cinders to be very expensive.

I believe it would be a very hard job to insulate properly a concrete tank. Certainly it would cost more to insulate than would an iron or wooden tank. As a matter of fact I do not know where to find a concrete tank that is properly insulated. The claim that

has always been made to me by its advocates is that it is cheaper than a properly insulated iron or wooden tank, but, when both are properly insulated, all my estimates show the concrete tank to be far more costly than iron or wooden tanks.

A freezing-tank should be kept clear of the ground if it is possible to do so, and should always be properly insulated. Any slighting of the tank insulation will add materially to the expense of operation, no matter what the tank is made of.

N. H. Hiller.—I would like to ask Mr. Torrance about what the cost per square foot is, basing it on cement costing, say, $1.40 a barrel, including insulation. Has he any figures?

William M. Torrance.—The tanks that I had reference to, tanks I know were built that way, cost the contractor that made them about 60 cents a square foot, including the insulation—8 inches of cinders and tar. The contract price, of course, was somewhat higher than that. It included the guarantee as to the concrete.

So far as the arguments about whether to use cinders and tar for insulation are concerned, I must confess that I cannot speak from such a great lot of experience, but in those that I saw built I know that no water will go into the cinders and tar to freeze them up, because there is no place for water. The cinders were filled with tar, they were not left partially open, to become filled with water later. Concrete tanks, of course, have failed, no doubt. Concrete tanks will continue to fail if they are not built right. Of course, there is the last chance of the personal equation. Anybody who makes concrete tanks must make them right. Of course, it is very evident on the face of it that you cannot get a chance to inspect the concrete work afterward as you can get a chance to inspect steel afterward, but it must be done and done rightly, and that is all there is to it, or else it should not be done at all. I shall not argue with anybody about that point. If you cannot afford to do it, and have it done properly, it ought not to be done. The cost of concrete, well made, I contend, can enter with steel on a fair, even basis, and it will cost less, if anything, than the steel, if both are well made and both properly insulated.

Irving Warner.—Mr. Torrance, if instead of having a tank for your can system, as this sketch is intended for, you were to make a tank for a plate plant, in which you had a long tank, perhaps 100 feet long and 30 feet wide, which tank was to be divided by bulkheads into a number of smaller tanks, each of which would be, maybe, 10 feet of this total length, would you make that entirely monolithically? Do you think you could make it satisfactorily in

one mass or would you prefer to make it as a number of separate tanks, each one 30 by 10 feet, with proper height?

William M. Torrance.—In answer to that question, I think that would depend a great deal on local conditions. If it were possible to do so I should by all means make the whole thing in one mass. If local conditions are such that you cannot get the materials to the place fast enough to make sure you could do it, it would be better to separate it and make some of those cross walls double and make it in separate tanks, as you say. But there is no physical impossibility about making such a structure in one mass.

Irving Warner.—Mr. President, I was very much interested in what Mr. Shipley said about calcium chloride being no better than sodium chloride. Why this struck me so particularly was because at our plant we are using calcium chloride for melting off the ends and bottoms of our plates. We use the direct expansion system, and use hot gas in melting off the plate proper. We bought calcium chloride, and though we bought the best there was; yet in spite of that there seemed to be a great deal of solid residue that had a tendency to clog up our pipes, until we put a separate tank on for letting that material settle out. Our engineer insists that it is corroding his pipes, though so far we have had no trouble. So I would like to have a few more words from Mr. Shipley on that matter as to what he thinks is the proper scheme. I have heard so much about calcium chloride; that it is just like oil to clean corroded metal; I would like to know some more about those qualities.

Thomas Shipley.—All I can say about it is that complaints have been coming to us for several years from plants using chloride of calcium. I find, from the reports we receive from those plants, that the calcium seems to attack cast iron more than it does wrought iron. For instance, tees and elbows and brine ends of the pumps become soft and are eaten away before the wrought pipe is affected. This, as you may imagine, is a very serious condition of affairs. In a plant of which our company is the owner we have replaced a brine pump twice in five years, but, strange to say, the fittings and pipe connected to this pump have not suffered to any great extent from the action of the calcium. Our chemists are working to see if they can find the cause of this trouble, but as yet we have not been able to discover anything definite.

N. H. Hiller.—There are a number of causes for these troubles. The principal cause is acid calcium. Oftentimes calcium chloride is not entirely neutral. It is very difficult to make it perfectly neutral, and not have it slightly acid or slightly alkaline. Alkaline is

preferable, but often, when it is heated up in mixing it at the start, an acid condition arises which will continue that way. The Carbondale Calcium Company sold a few years ago a substance known as "Kal-Krystal," which was good for use in automobiles as a non-freezing compound. The difficulty with it, in certain places like Pittsburg, was this: Monongahela River water was used in dissolving, and it had sufficient free sulphuric acid in it—a very small quantity—I think about three grains to the gallon, possibly a little more, to make the calcium acid, and then an electrolytic action took place between the copper, tin and the iron of the cylinders. Probably a good many of these difficulties would be eliminated if either distilled water or a good quality of water was used to melt the calcium in the first place.

Irving Warner.—What would you recommend to make that solution thoroughly neutral? Is there any alkali you can add?

N. H. Hiller.—A little caustic soda; not very much, because it will cause a froth if you use too much.

Irving Warner.—Would you use a litmus paper test on it?

N. H. Hiller.—I do not think the litmus paper test would do. You cannot get a litmus paper test that is sufficiently sensitive. Phenolthalin will indicate it in all conditions. In the first place, you do not have to use heat to dissolve calcium. A great many people boil it. But instead of doing that, put it in a dissolving barrel in the brine pump discharge, and that will help to prevent corrosion. But the principal thing is to see that your water contains no acid whatever.

Albert Johnson.—May I ask Mr. Hiller a question? I understand that there are two processes by which the calcium chloride is produced, one a natural process, the residue from salt mines, and another a chemical process known as the Solvay process. I would like to know, if you are familiar with that, which would produce the purer quality. There is also another process which I find in Michigan, where they extract the bromide by electricity. I want to know if this does not purify the calcium.

N. H. Hiller.—If the article is calcium chloride it is calcium chloride; but it may be slightly acid or alkaline. It is preferable to have it alkaline. As a rule, the calcium produced at the different bromine works contains more or less salt, though the salt is being extracted now and sold as a by-product. It also contains magnesium chloride as well as calcium chloride. It is a question whether magnesium chloride produces any action. Experiments are being conducted by the Solvay Process Company to establish that fact, but it

is a very slow process, and they are giving it a good year's trial under different conditions. We know magnesium chloride is corrosive at high temperatures, and it is very likely that if any magnesium chloride is present and the calcium is heated in order to dissolve it, a certain action starts in.

Albert Johnson.—I know a firm in Kansas City that dissolved two different makes of calcium chloride in bottles, and put a piece of iron in each bottle, a nail, we will say, and let it remain there for two or three months, and it was found that the action on one nail was ever so much greater than the action on the other; so there must be a great difference in the quality of the calcium, and when we say calcium chloride that does not say anything for purity, because there are so many different impurities in calcium, and I think it ought to be purchased by chemical analysis.

Thomas Shipley.—I believe the proper way to purchase chloride of calcium is by analysis. In that way we may get a line on the cause of the troubles we are having with it; at least we would have a basis to start on.

This subject is a very important one to all the users of ice-making and refrigerating machinery, and it should receive the special attention of all the makers of calcium as well as the builders of the machinery. The York Manufacturing Company is spending considerable money on the causes of the troubles with calcium, and we hope to soon get at something definite.

If the trouble is not soon located, it will cause a great falling off in the use of calcium chloride, for instead of calcium chloride being the preservative of iron, as we have been led to believe it is by the circulars and claims of the manufacturers, it is proving itself to be a very treacherous substance, which may at any time destroy our plants and cause no end of loss.

The President.—Has this trouble come from certain localities?

Thomas Shipley.—No, it is all over, where they use calcium chloride.

The President.—Irrespective of the water?

Thomas Shipley.—Yes. I tried to find out whether it was a particular brand of calcium, but cannot find any difference. In one place one kind is used and in another place another kind.

The President.—Speaking of the action of water on cast iron, I built not long ago two 16-inch centrifugal pumps. They were used in Pittsburg. They lasted about six months, and were completely eaten up. They replaced two pumps that had been eaten out before, and they had built about 2 feet of cement around the

pump as a patch so that if the chloride of calcium was dissolved in the water it certainly would not neutralize the acid action of this kind of water on the cast iron.

Theodore Kolischer.—My experience with calcium chloride was very disastrous. It was with an ice-making plant in this country, which was originally operated with salt brine. Later on it was changed to calcium. Inside, I believe, of one year or fifteen months, there was a deterioration of the pipes, and, in fact, of the tanks, that was most ruinous to the plant. Upon listening carefully to this discussion it occurs to me that during my travels and inspections in Europe I found that a great many, if not a majority, of the plants abroad have been operated with chloride of calcium for a great many years without the disastrous effects discussed this evening. It seems to me that the trouble is right here in this country. Whether that is due to the water, as Mr. Hiller explained, or to the different qualities or compositions of calcium, should be taken very strongly into consideration and sifted down to the bottom, because, as Mr. Shipley says, it is a very important and vital question. If in Europe they manage to operate with chloride of calcium with success, why should not we be able to do the same thing? So after all it seems to me it is the difference in the various qualities of calcium chloride, and that should be thoroughly discussed.

N. H. Hiller.—Mr. President, on the question of salt brine that Mr. Kolischer brings out, I would like to say that I know of two plants where salt was used originally, and then shifted over to calcium. That is the point, whether these other salts, in conjunction with calcium, have any bad effect on the brine system. Of course, being interested in a calcium company, I do not want to make a statement at present that I cannot support; but what I want to say is that this question is being investigated very carefully, and as soon as any definite conclusion is reached, I will be glad to submit it to the Society.

There are several plants in New York city running with calcium chloride, one since 1892. I think one of the old Merchants' houses is still running with the same pipes installed in 1892 or 1893. It seems that local conditions cause the trouble; and as soon as we have something tangible we will be glad to submit it.

Henry Torrance, Jr.—I happen to know of a brine cooler that is in operation today that was put in in 1884, which is in good shape. It has always used calcium. There are very few cases where any of the pipes have corroded. I remember but one or two, possibly one or two fittings in the whole plant.

Thomas Shipley.—Mr. President, I do not say that wherever chloride of calcium has been used there has been trouble; but I do say that we are beginning to experience a great deal more trouble from plants using chloride of calcium than we do from plants using salt brine, and I believe, as some of these gentlemen say, it is a local proposition. But we ought to be warned what to do or how to protect ourselves against conditions that are so destructive to our plants, and not rest in supposed security, believing that we have something that will prevent just the thing that is likely to happen. We have all been led to believe that chloride of calcium was a preservative of iron. We have never been told that we ought to use distilled water with it or keep acid away from it; hence it cannot be said that we have not used all the precaution that we had knowledge of in our use of it. If the manufacturers of calcium chloride knew that it should be used only with water containing no acid, or that brass-lined pumps should not be used, then they deserve censure for not notifying all users of their goods of these facts.

No. 57.

PERFORMANCE OF AMMONIA COMPRESSION MACHINES.

By Charles Edward Lucke, New York, N. Y.

(Member of the Society.)

Ever since the ammonia compression system of refrigeration became one of the recognized standards, and apparatus of considerable variety of form came into competition under the system, there have been many spirited discussions on the relative advantages of the different structural details. Especially true is this with regard to the compressors themselves, in which field there is probably a greater range of forms and claims of superiority than for any other single part of the total equipment. One of the most common units of comparison applied to compressors is the horse power per ton of refrigeration, and on this basis performances of machines have been recorded and compared, the superiority of one over another expressed in this unit being ascribed to all sorts of things, ranging from a small detail of structure to the complete system as a whole. As a matter of fact differences in performance expressed in this unit were often charged to influences that could not have produced them; but however absurd some of these conclusions may appear after analysis, it must be admitted that under the circumstances they were not unnatural. When the performance of any machine or manufacturing system, such as a textile mill, a power plant, a machine tool or a steam boiler, is the combined effect of a large number of variables, some of which can be, but often are not, evaluated, and others equally important can never be, then the solitary effect of any one may easily be misconstrued. Tests may be made with all possible honesty of intention for the purpose of proving the superiority of a furnace grate in a boiler or a cost system in a shop, but unless the problem be first analyzed to find all the really essential variables, and positive care exercised to keep all of them constant except the one under observation, then the effect of that one cause can never be known. The only sure way of thus isolating a single variable is the recording of data on all the variables the constancy of which is desired, to prove that such constancy has or has not been obtained.

The general process then of isolating a single cause in relation to its effect will involve certain successive steps. The possible causes must by analysis be found and divided into, first, those fundamental causes, often called theoretical because they are susceptible of evaluation by computation without test; second, accidental interferences due to details of structure, and, third, those unavoidable interferences due to operating conditions. This method of approach will permit of the laying out of a test in such a manner as will permit of a judgment of the precise single cause of any observed combined effects.

Horse Power Per Ton of Refrigeration as a Unit of Comparison.

In the case of the compressor horse power per ton of refrigeration it can be shown that the result is dependent fundamentally on (*a*) the density of vapor at the beginning of compression in the cylinder; (*b*) the available B. T. U. cooling effect per pound of ammonia at the coils or cooler; (*c*) the initial and (*d*) the final absolute pressures in the cylinder. It can also be shown that the result is not at all dependent upon the structure of the compressor, its clearance, or the structure of the rest of the system. The symbolic expression for this statement is as follows:

Ammonia cylinder horse power per ton of refrigeration =

$$\frac{.87\, p_a}{W_c} \frac{n}{L_c} \frac{n-1}{} \left[\left(\frac{p_b}{p_a} \right)^{\frac{n-1}{n}} - 1 \right]$$

where p_a = absolute pressure in the cylinder before compression begins;

p_b = absolute pressure in the cylinder at the end of compression;

W_c = the weight per cubic foot of ammonia vapor (drawn from the cooler) as it exists in the cylinder before compression begins;

L_c = the available B. T. U. cooling effect per pound of ammonia at the coils or cooler, which is equal to the latent heat for the pressure in addition to the heat of vapor superheat, less the heat of liquid superheat;

n = the exponent of the volume in the equation for the compression line $pv^n = c$ and itself a constant for all initially dry saturated vapor or superheated vapor, but becomes variable for vapor initially wet, as can best be proved by the temperature entropy analysis.

Other Variables.

When, however, the problem of comparison is reduced to the conditions of suction pipe pressure and condenser pressure rather than ammonia cylinder pressures, there would be introduced two more variables, expressing pressure drop between the points of

measuring these new pressures and the cylinder. For two different compressors similarly erected these differences reduce to the pressure drop through the suction and discharge valves. Similarly, the exponent "*n*" might differ for the two machines if one used wet and the other dry vapor, but it would not appreciably differ, as has been at times supposed, with attempts to cool the ammonia in compression by water jackets or oil injection. It can, however, be shown that the variation in "*n*," due to initial wetness, is small, even for a considerable amount of liquid present; so small as to be generally negligible in comparison with other influences, such as valve resistance, for example.

Where one machine is operated with liquid injection into the compressor suction an additional variable is introduced with a double effect. First, there is a reduction in the quantity of vapor from the cooler that can be drawn from the suction pipe by an amount equal to the vapor produced by the liquid injection into it, and, second, there is a possible increase in this quantity due to the reduction of cylinder heating effect during suction, which double effects may or may not balance, depending probably upon the amount used. If, as a result of this liquid injection, there remains some liquid at the end of suction, then the effect will be to cause a variation in "*n*," previously noted.

It appears, therefore, from this analysis that the comparative performance reduced to indicated ammonia horse power per ton of two machines, one operating without liquid injection and the other with it, will be dependent upon the amount of this liquid injection and on the pressure drop through the valves, in addition to those other quantities which are the same for the two machines. When, finally, steam cylinder horse power per ton of refrigeration be introduced as the unit of comparison the friction losses in the two machines give a third variable.

Structural details, therefore, enter into this comparative performance only as they may affect the friction in the machine, the suction and discharge valve resistance and the operative conditions as they may be affected by the liquid injection into the suction, assuming the other prime variables identical for the two machines. Tests of machines to indicate the precise effect of any one of these variables in the manner previously indicated are not on record, but a recent test made under circumstances somewhat unusual furnished a large amount of data, and it is the object of this paper to present these data and analyze the results.

While it is true these tests were not conducted so as to permit

the determination of the precise cause of differences, yet an interpretation may be made within certain limits and of sufficient value to warrant presentation, while analysis may show the possibility of misinterpretation.

Conditions of the Rochester Tests.

At the Kodak Park Works of the Eastman Kodak Company, Rochester, N. Y., in the early spring of this year there was conducted a comparative test of two machines, one a De La Vergne oil compressor, consisting of two vertical, double-acting ammonia cylinders, 11¼x22 inches, having a piston rod of 2½ inches diameter, driven by a single Corliss steam cylinder, 22x22 inches, with a piston rod of 3⅝ inches, and operated with liquid ammonia injection into the suction pipe near the compressor. This machine has been in service for seventeen years continuously, and was rated at 50 tons capacity at 45 R. P. M. The other machine tested involved the same steam cylinder and frame, but with the ammonia cylinders replaced by two York single-acting compressors, 15x22 inches, supposedly of equal rating. The object of the test was to find out the steam cylinder horse power per ton of refrigeration for the two machines under certain specified conditions without any analysis of the reasons for such differences as might appear. These conditions were a back or suction pressure of 20 pounds, gauge, a condenser pressure of 185 pounds, gauge, the tonnage to be determined by and expressed in terms of brine cooled by accurate tests of its specific heat and density, temperature drop and quantity, and the speeds to be three runs, at 45 R. P. M., one run at a lower and two runs at higher speeds, at the option of the operators. The brine outlet temperature from the cooler was to be kept constant for all runs.

There were thus six tests of ten hours each for each machine; the De La Vergne runs were made on February 2, 3, 4, 5, 6 and 7, and the York on March 6, 7, 8, 9, 10 and 11. The observations made were checked at the time recorded by representatives of both builders and the Eastman Kodak Company, and these observations are presented in Tables I to XII, inclusive, one table for each day.

These tables also contain the areas of the indicator cards, which were measured by Charles Main, of the firm of Dean & Main, Boston, Mass., as a disinterested party. Each column of figures was added and the averages for each day were found correctly by the Eastman Kodak Company, except for the brine temperature corrections on March 10, where the figures $\left\{\begin{matrix}27.85\\15.53\\11.82\end{matrix}\right\}$ reported for columns $\left\{\begin{matrix}10\\11\\12\end{matrix}\right\}$ should read $\left\{\begin{matrix}27.23\\15.55\\11.78\end{matrix}\right\}$. Temperatures were taken in the usual way by mercury thermometers in wells which were afterwards calibrated by the Taylor Instrument Company, from whose report the following corrections for the average observed temperatures are derived:

For thermometer No. 5	0° F.
" " " 6	—.15° F.
" " " 7	—.1° F.
" " " 8	0° F.
" " " 9	—.5° F.
" " " 10	0° F.
" " " 11	— 3° F.

These thermometer numbers correspond to the column numbers in the tables of observations. The specific heat of brine used in determining the tonnage was found by Prof. William Hallock of Columbia University by electrically heated Dewar bulbs for certain ranges of temperatures, and the value of .678 was agreed upon as representing as nearly as possible the value at the temperatures observed. The brine was measured by the revolutions of an inverted motor-driven triplex pump, calibrated by discharging into weighing tanks, and the weight of brine per revolution was then fixed at 41.15 pounds. For the purpose of reduction to standard conditions it was agreed to accept 1.1 as the specific heat of liquid ammonia and 95° liquid temperature as a standard at the expansion valve, this corresponding approximately to saturated vapor for the prescribed 185 pounds condenser pressure. The method of reduction to standard conditions, printed at the bottom of the Eastman result sheet, Table XIII, was not agreed upon, but is that used by it. The mean effective pressures derived from the indicator cards and reported on the same table are based upon an assumed correct indicator spring of 60 pounds scale, and on card lengths, indicated below Table XIII, which were not the same for the two ends, probably

because of unequal lengths of cord and angles of lead to the cross-head. The indicators used were new, and by some error in sending them to the American Steam Gauge and Valve Company, Boston, Mass., for calibration they were removed from the cylinders in which they were used and tested in other cylinders, which may account for the difference between the calibrations reported, of which there were three in number and no two agreeing. Horse power results are, therefore, likely to be in error on this account. While it is true that a seasoned spring is not likely to change its scale during a test of this character, this can hardly be said to be true of a new spring. If the two calibrations, that after the De La Vergne test and the first after the York test, were actually correct, then the horse power correction that should follow would favor the De La Vergne machine to the extent of about 1 pound in 60, or 1.7 per cent. for the head end and something less than 1 per cent. for the crank end.

Method of Determining Results.

The method used in determining results was always that of arithmetical averages. As certain suction and discharge pressures were prescribed and could not always be maintained the arithmetical averages of the pressures observed at the time of reading were accepted as an equivalent in effect to a constant value for the whole time. If variations of suction pressure, for example, are not large or abrupt, it is pretty certain that the mean value may be so determined, and that the results corresponding to this mean may be assumed with justice to be the same as might have been obtained with perpetual constancy of the pressure at the mean value. When, however, there is a considerable fluctuation of one of the conditions such as this it is open to serious question whether the result is the same as would be obtained with actual constancy of that condition equal to the mean. Comparisons which might justly be made between two machines when the fluctuation of conditions beyond the control of the operators are the same in kind and amount might be quite impossible or unfair when they are not the same. Such was the case for these test runs.

The Cooling Load.

The cooling load for the tests was an artificial one, provided by the York Company and guaranteed by it to be adequate, in the form of a steam brine heater and double-pipe exchanger. Cold brine from the cooler was circulated through a double-pipe system on the other side of which warm brine, heated by steam coils, was

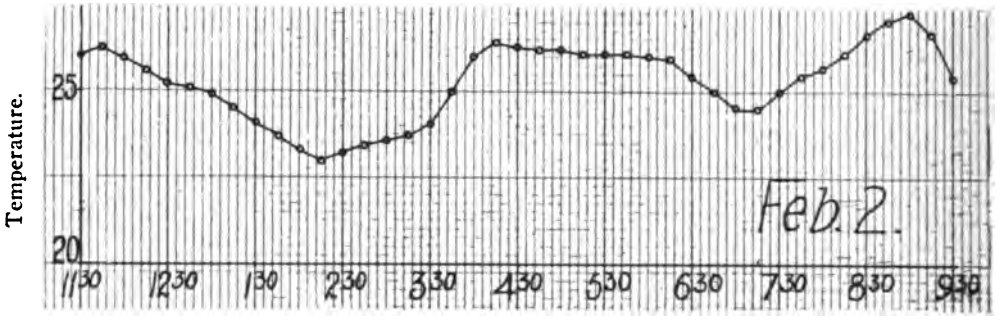
counter-circulated. It was found that during the De La Vergne series the steam surface was insufficient and the steam supply to the heater fluctuated most unexpectedly, so that the operator, finding his brine getting cooler, would open the steam valve, hoping to raise the temperature, whereas a lack of supply of steam permitted it to continue to drop, and then, without warning, steam would come, and before the valve could be adjusted again the brine had become too warm. As a consequence, therefore, during these runs the brine inlet to the cooler had a temperature impossible to control at will, and through no fault of the operators or builders of the machine. After the De La Vergne, and before the York tests, the conditions were improved materially, as is shown clearly in Fig. 1, which gives the brine inlet temperature readings plotted to a base line of time for the twelve days' test, the left-hand column showing the considerable fluctuation suffered by the De La Vergne machine and the right-hand column showing the surprising constancy enjoyed by the York machine. These fluctuations of brine inlet temperature are the causes of similar fluctuations of the brine range in the cooler, the suction pressure at the cooler and the temperature of the gas leaving the cooler. That these also fluctuated with and as a consequence of variable initial brine temperature, which was beyond the control of the operators, and fluctuated more for the De La Vergne series before the steam heater was improved than for the York series after the heater was improved, is shown clearly by the plotting of the readings taken for these quantities in Figs. 2, 3, 4. As the horse power per ton is very strongly dependent upon the conditions of suction gas, fluctuations of the kind suffered by the De La Vergne machine are serious, so that while nominally the machine operated at the average pressure indicated in the preceding columns of figures, actually it operated at considerably varying suction pressures and with considerable variation in the other quantities associated. The performance in horse power per ton is not as good as it would have been under conditions like those maintaining for the York series.

FIGURE 1.—BRINE TEMPERATURE AT COOLER INLET.

DE LA VERGNE.

DAY.

1.



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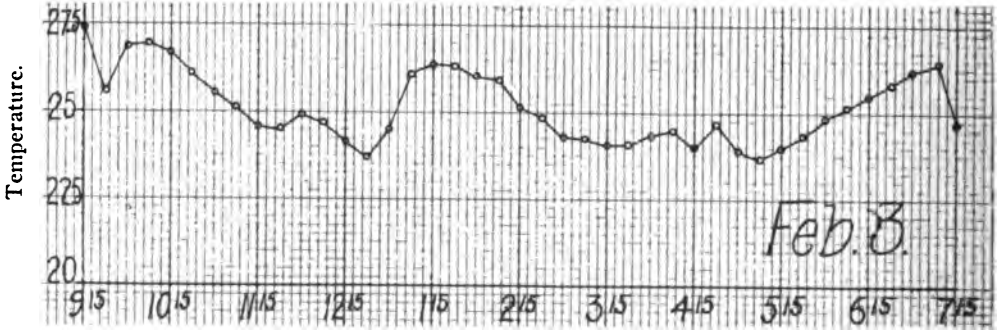


3.

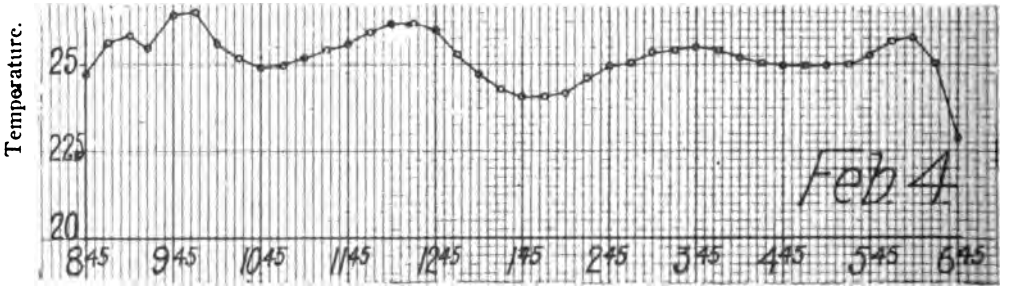
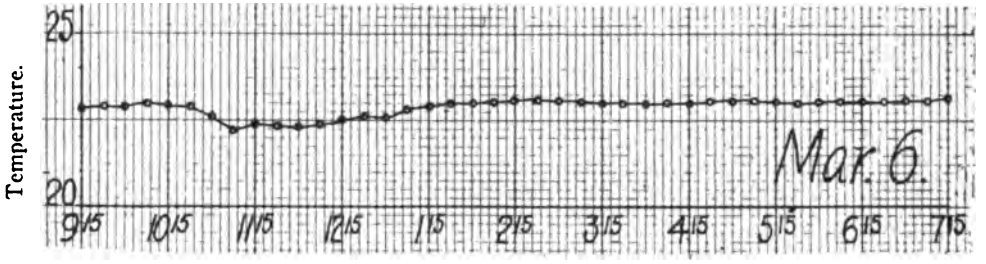


FIGURE 1.—BRINE TEMPERATURE AT COOLER INLET (CONTINUED).

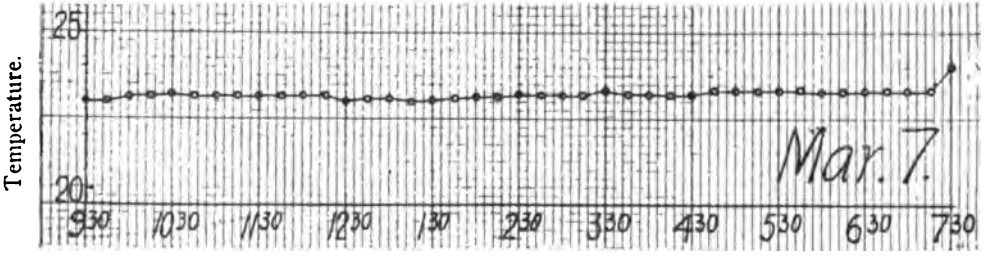
YORK.

1.



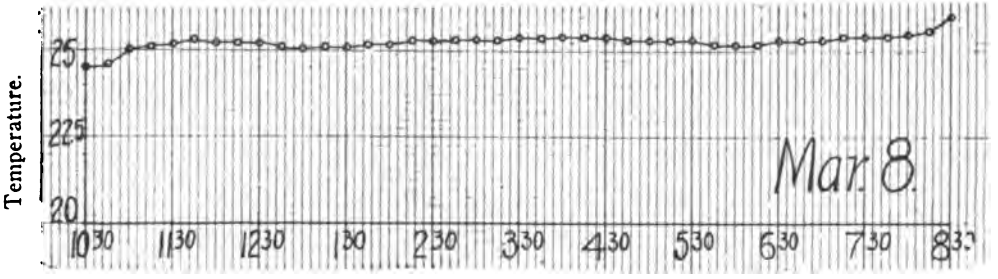
Time.

2.



Time.

3.



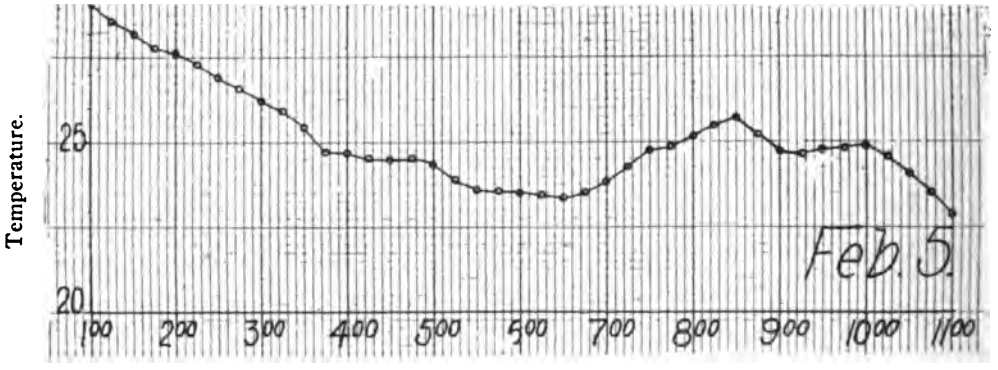
Time.

FIGURE 1.—BRINE TEMPERATURE AT COOLER INLET (CONTINUED).

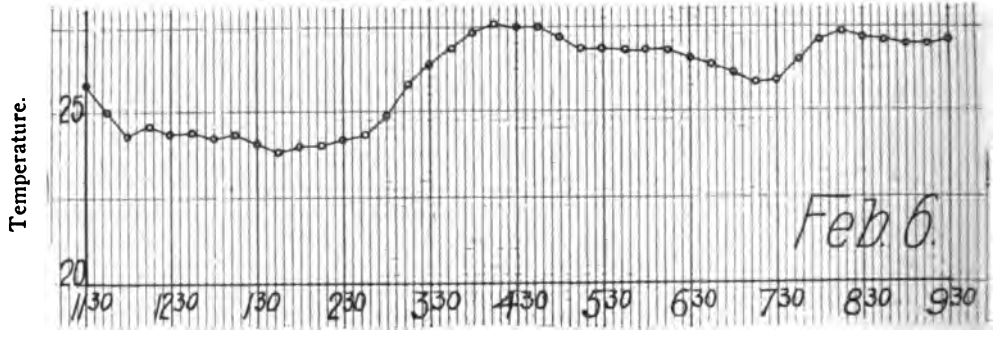
DE LA VERGNE.

DAY.

4.



5.



6.

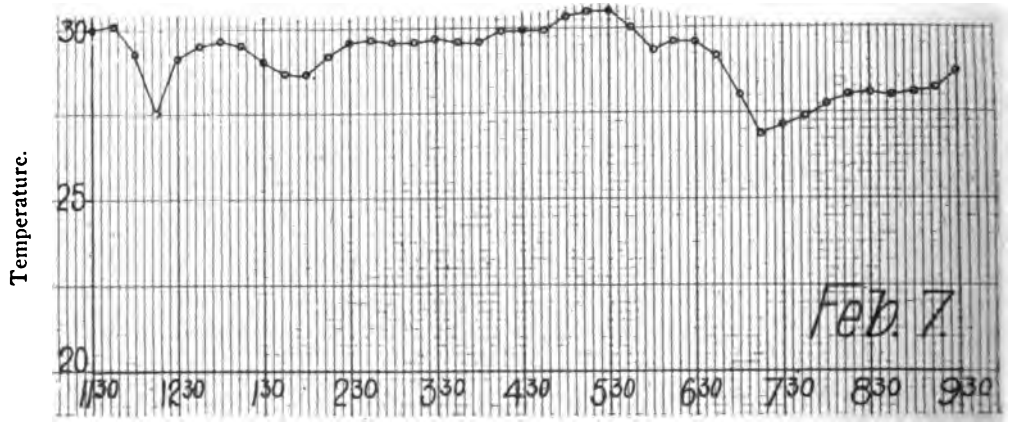


FIGURE 1.—BRINE TEMPERATURE AT COOLER INLET (CONTINUED).

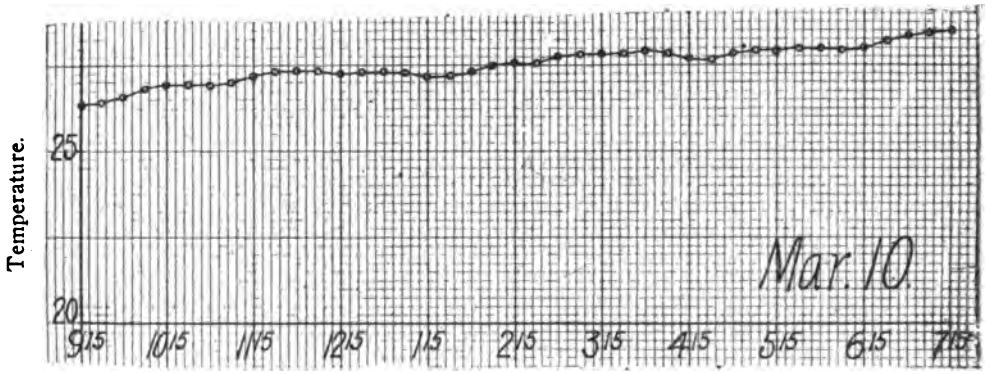
YORK.

DAY.

4.

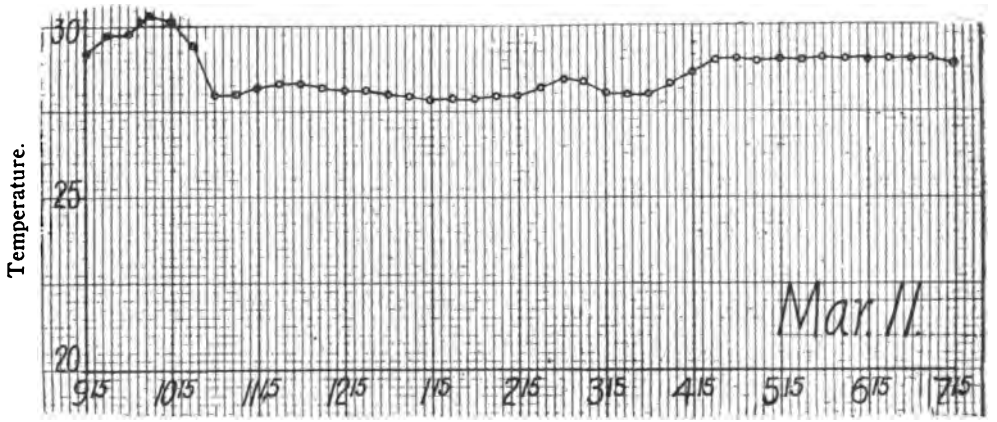
Time.

5.



Time.

6.



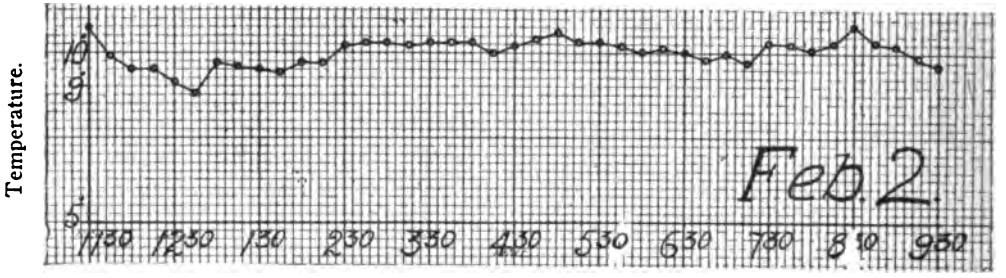
Time.

FIGURE 2.—BRINE TEMPERATURE RANGE IN COOLER.

DE LA VERGNE.

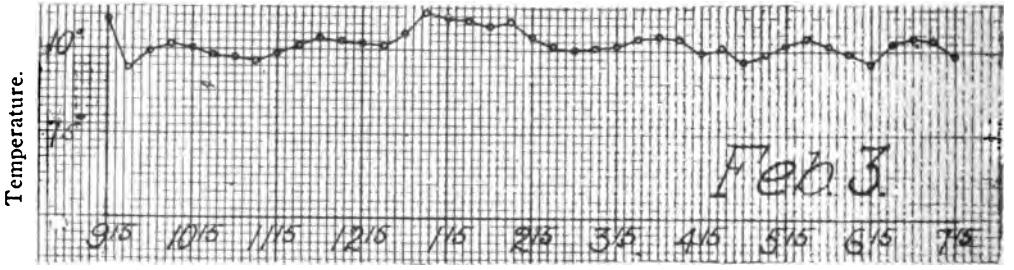
DAY.

1.



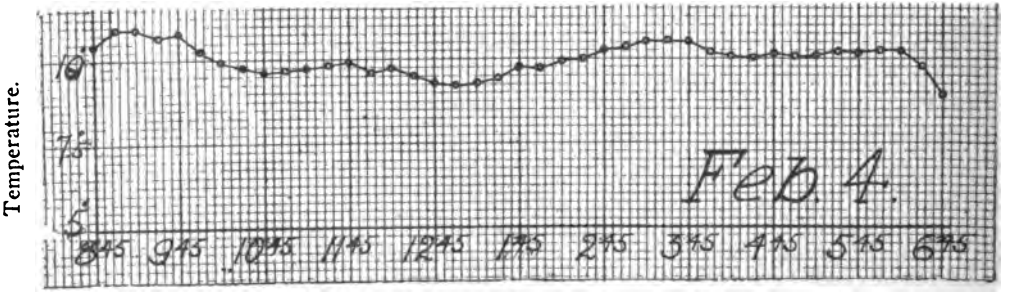
Time.

2.



Time.

3.



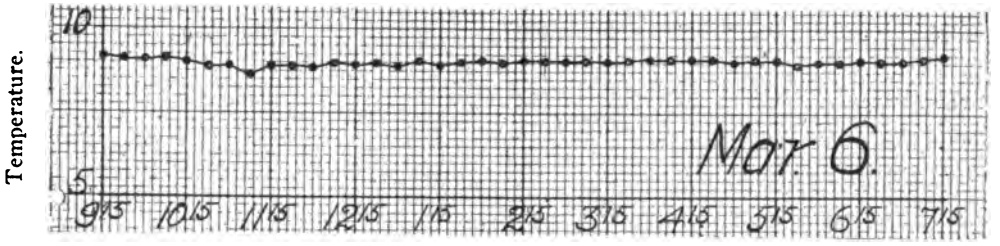
Time.

FIGURE 2.—BRINE TEMPERATURE RANGE IN COOLER (CONTINUED).

YORK.

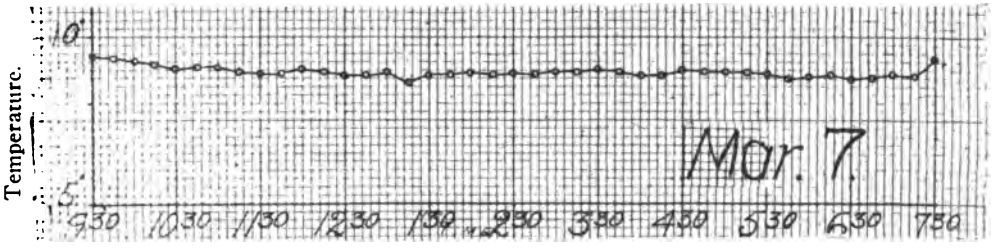
DAY.

1.



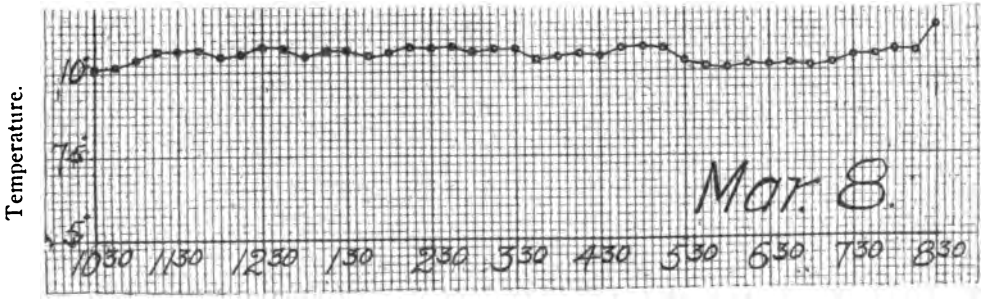
Time.

2.



Time.

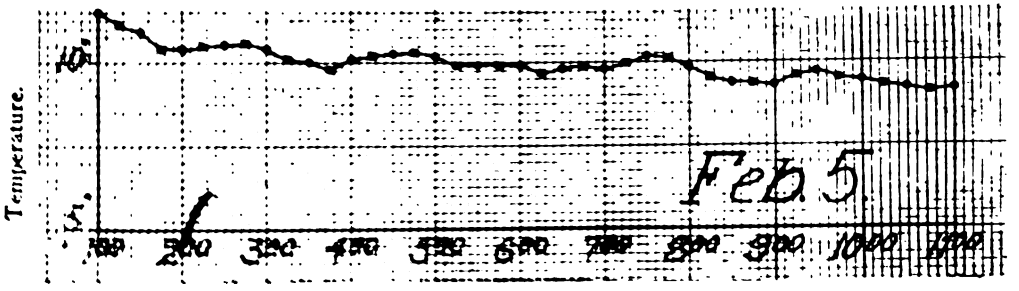
3.

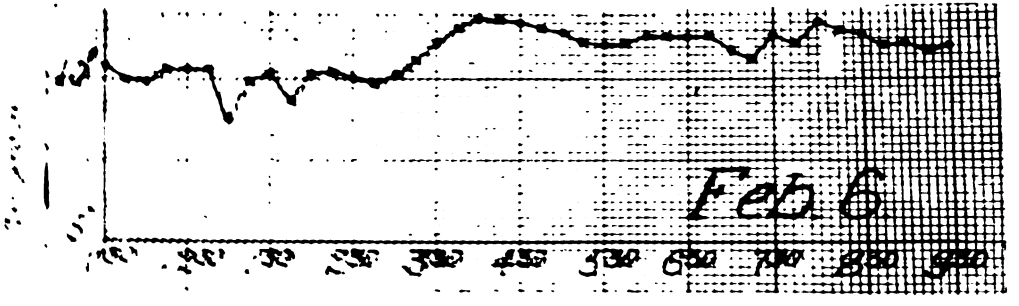


Time.

FIGURE 2—BRINE TEMPERATURE RANGE IN COOLER (CONTINUED).

DE LA VERGNE.





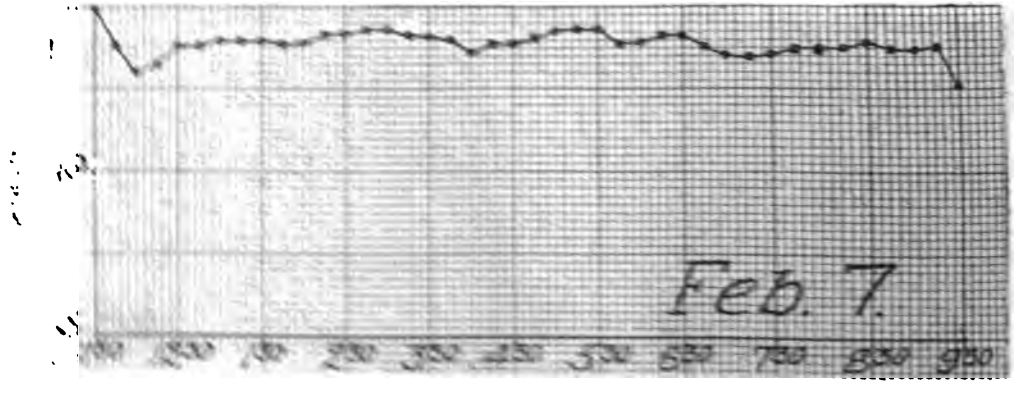
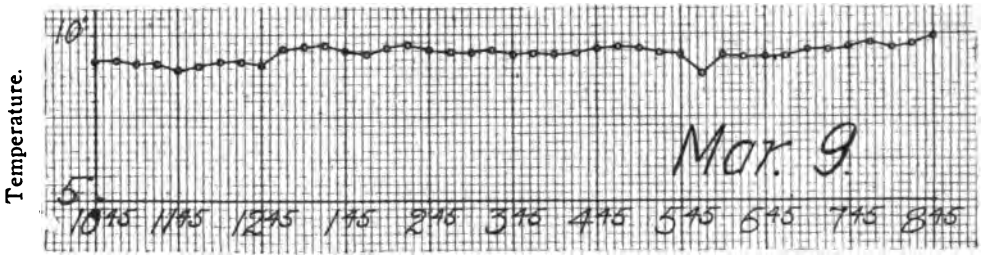


FIGURE 2.—BRINE TEMPERATURE RANGE IN COOLER (CONTINUED).

YORK.

DAY.

4.

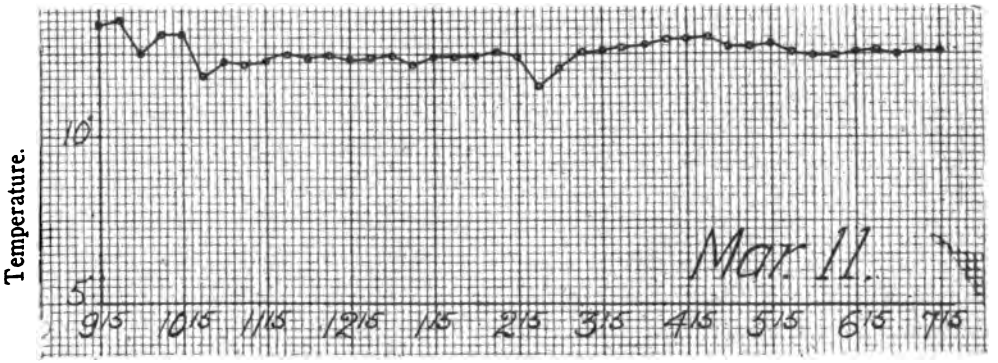


Time.

5.

Time.

6.

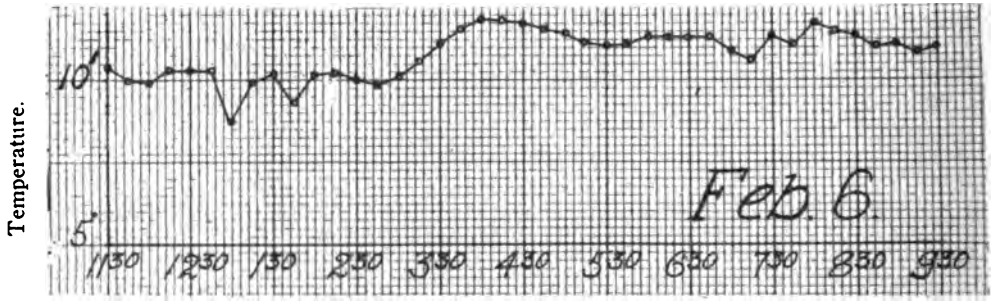


Time.

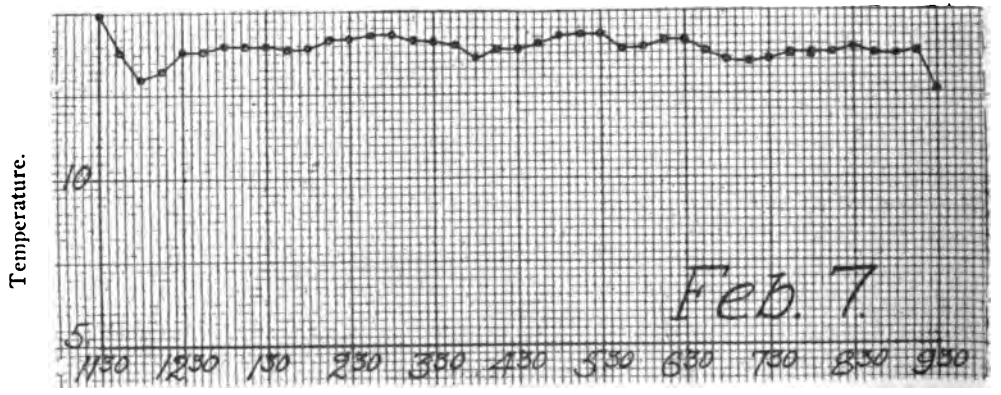
FIGURE 2.—BRINE TEMPERATURE RANGE IN COOLER (CONTINUED).

DE LA VERGNE.

Time.



Time.



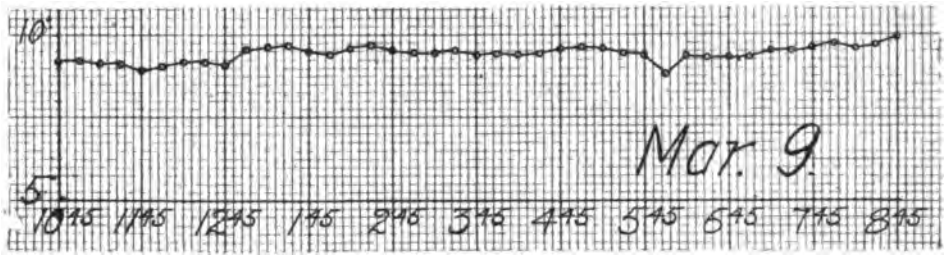
Time.

FIGURE 2.—BRINE TEMPERATURE RANGE IN COOLER (CONTINUED).

YORK.

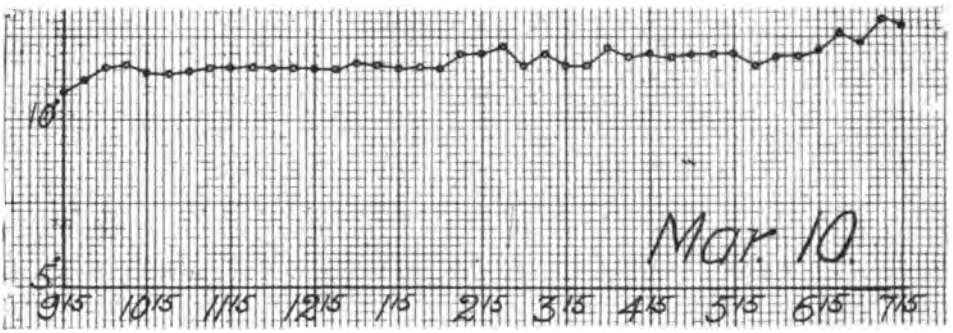
DAY.

4.



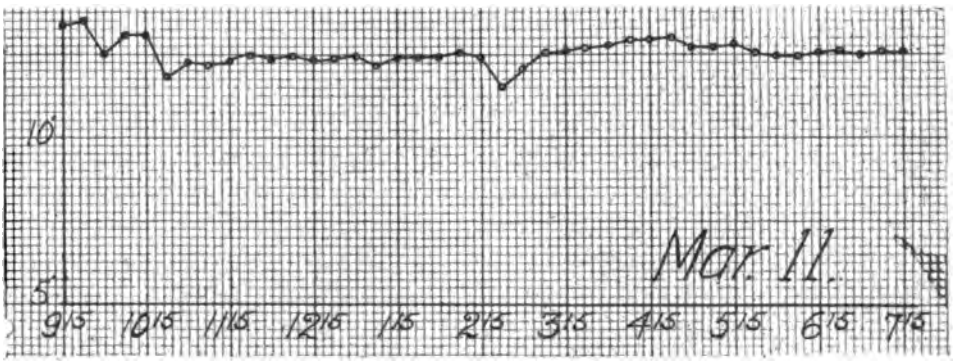
Time.

5.



Time.

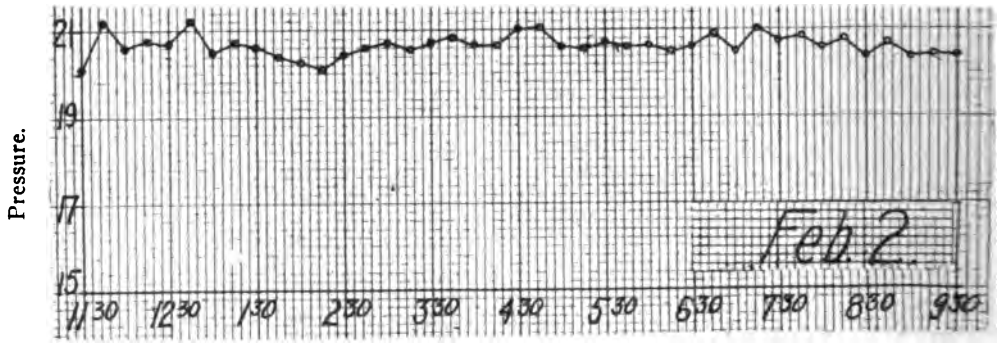
6.



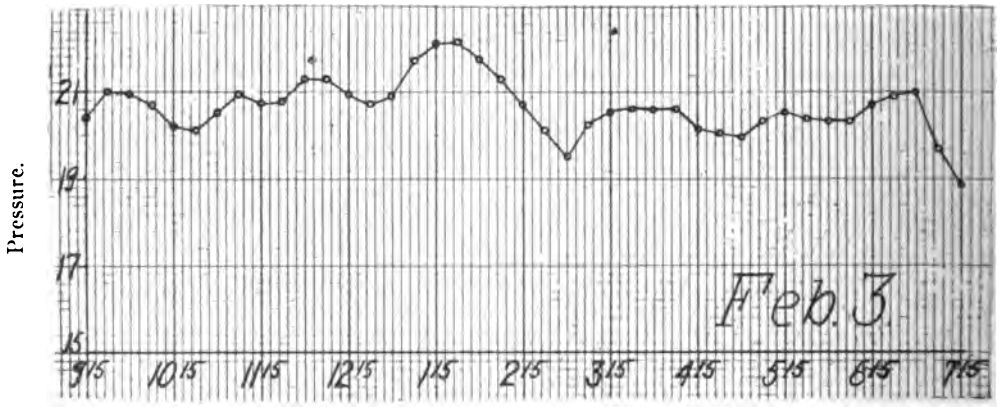
Time.

FIGURE 3.—SUCTION PRESSURE AT COOLER.

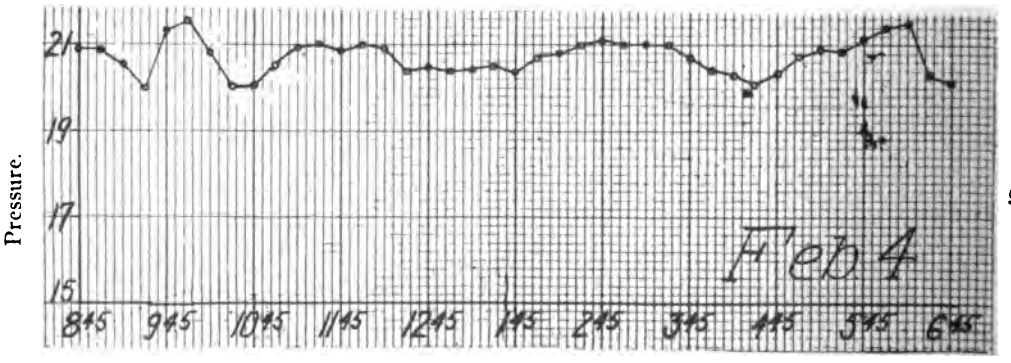
DE LA VERGNE.



Time.



Time.



Time.

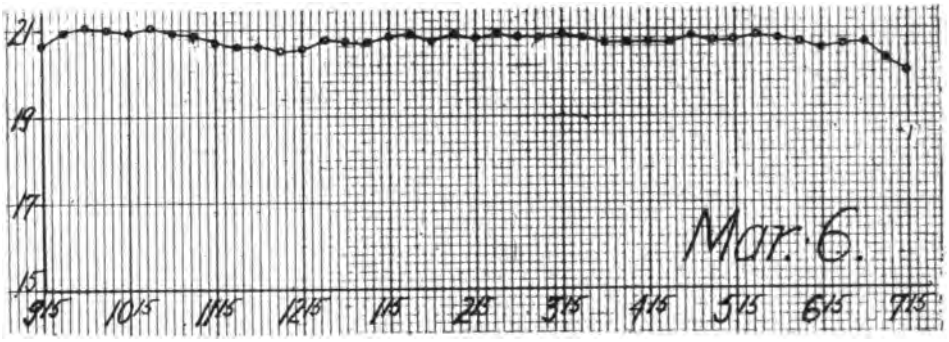
DAY. 1. 2. 3.

FIGURE 3.—SUCTION PRESSURE AT COOLER (CONTINUED).

YORK.

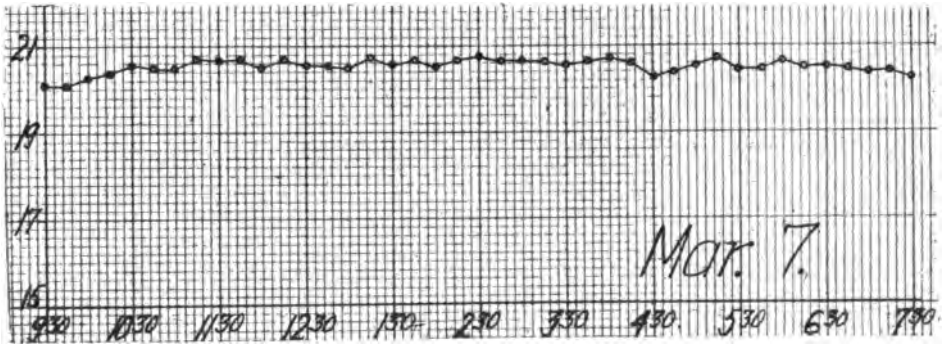
DAY.

1. Pressure.



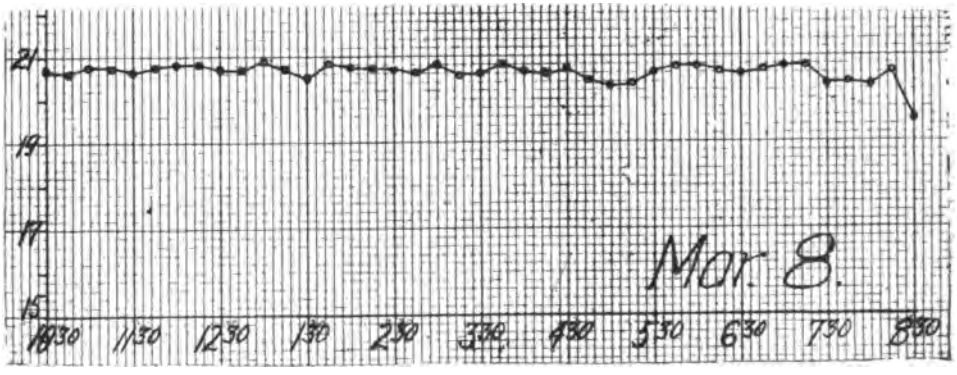
Time.

2. Pressure.



Time.

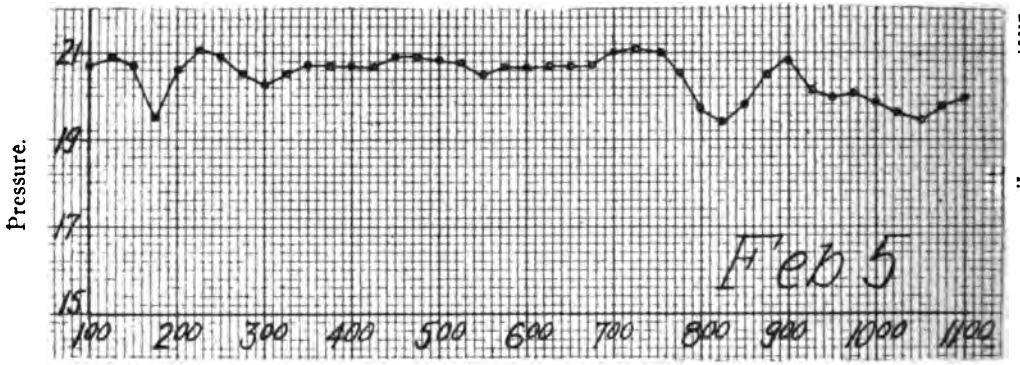
3. Pressure.

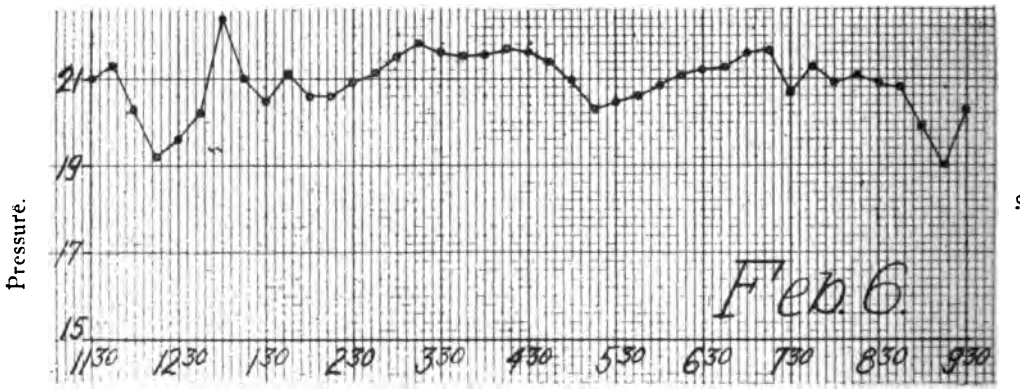


Time.

FIGURE 3.—SUCTION PRESSURE AT COOLER (CONTINUED).

DE LA VERGNE.





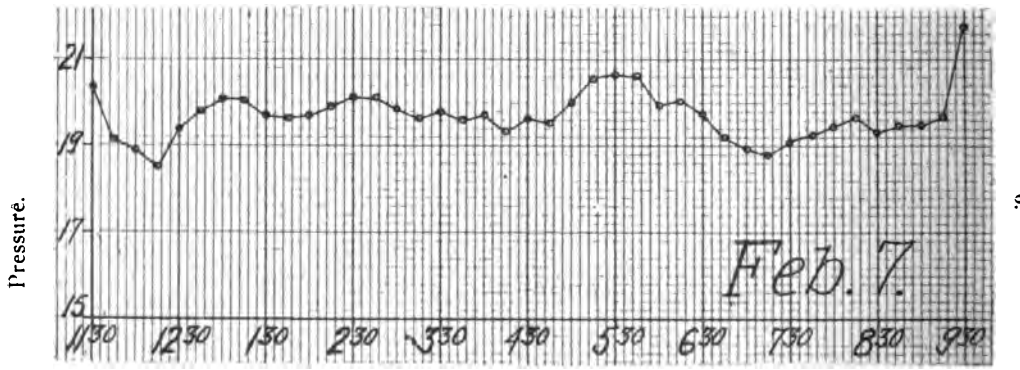
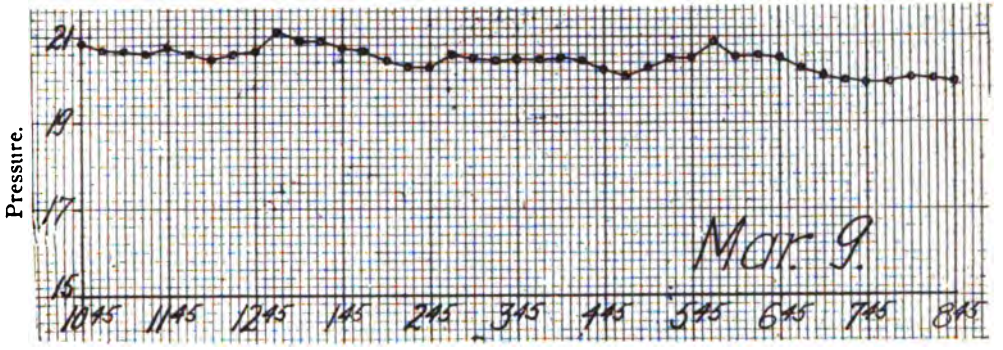


FIGURE 3.—SUCTION PRESSURE AT COOLER (CONTINUED).

YORK.

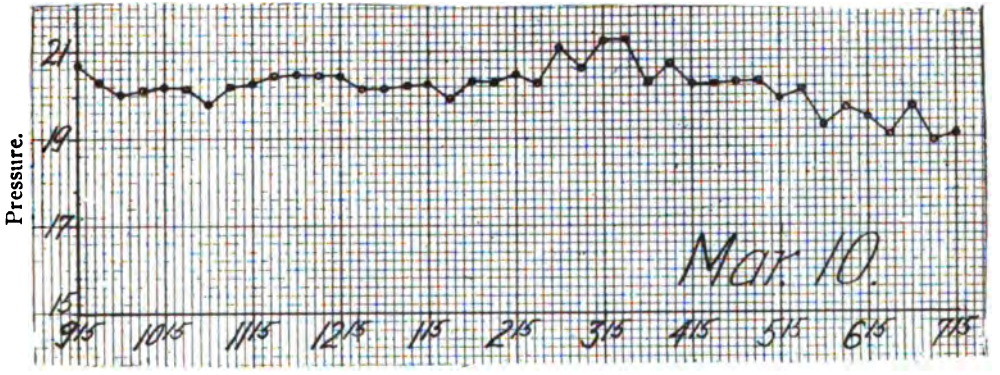
DAY.

4.



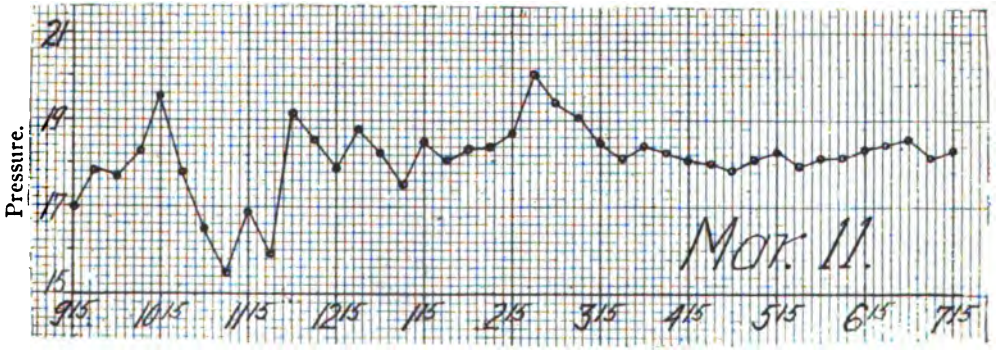
Time.

5.



Time.

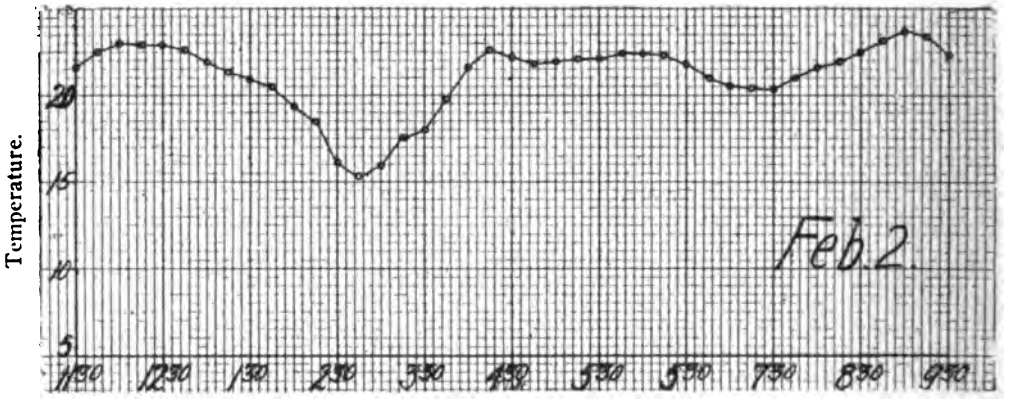
6.



Time.

FIGURE 4.—SUCTION TEMPERATURE AT COOLER.

DE LA VERGNE.



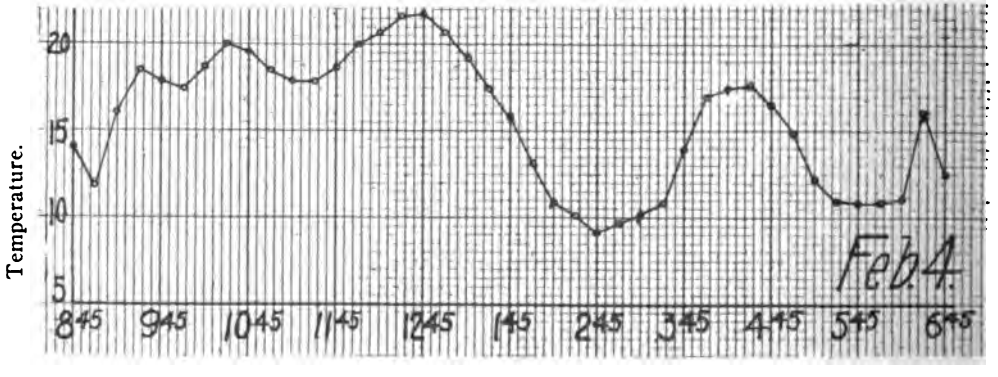
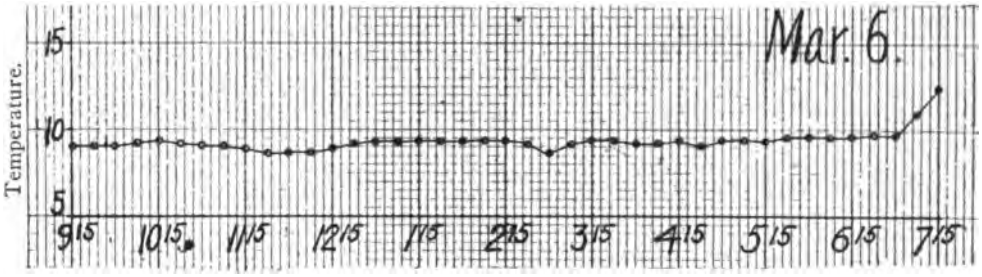


FIGURE 4.—SUCTION TEMPERATURE AT COOLER (CONTINUED).

YORK.

DAY.

1.

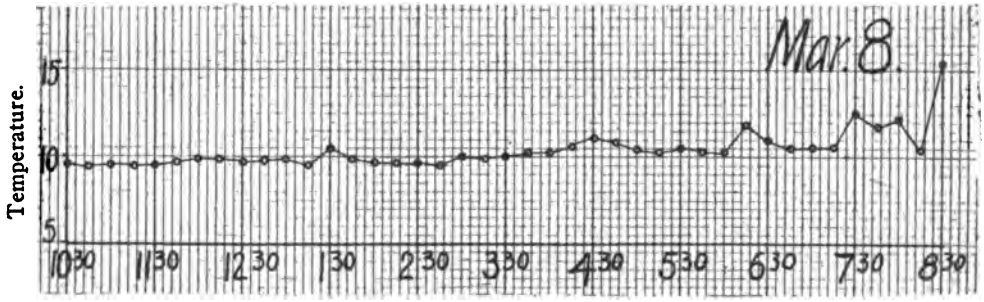


Time.

2.

Time.

3.



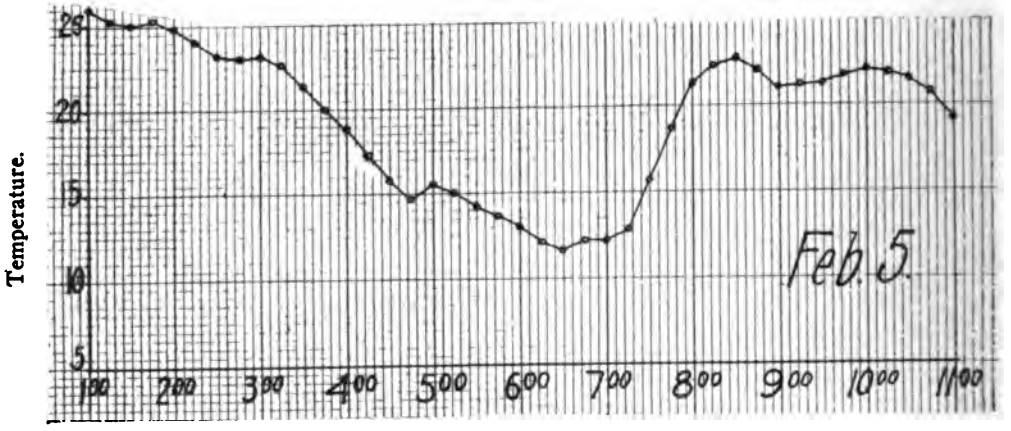
Time.

FIGURE 4.—SUCTION TEMPERATURE AT COOLER (CONTINUED).

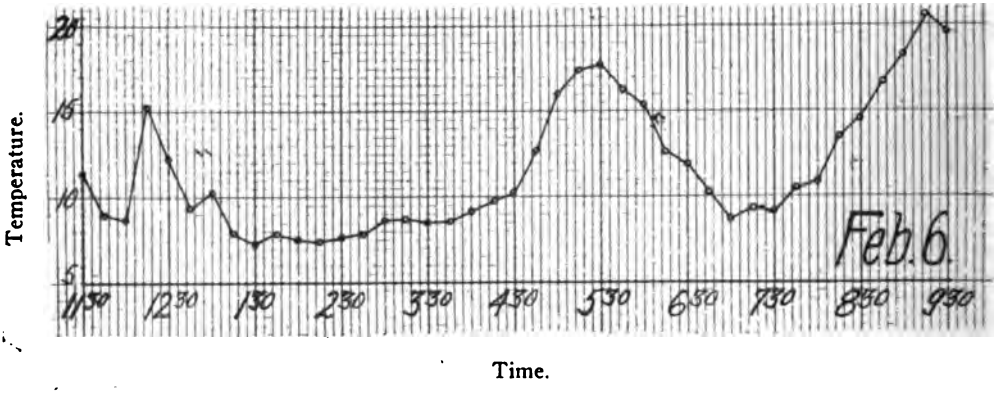
DE LA VERGNE.

DAY.

4.



5.



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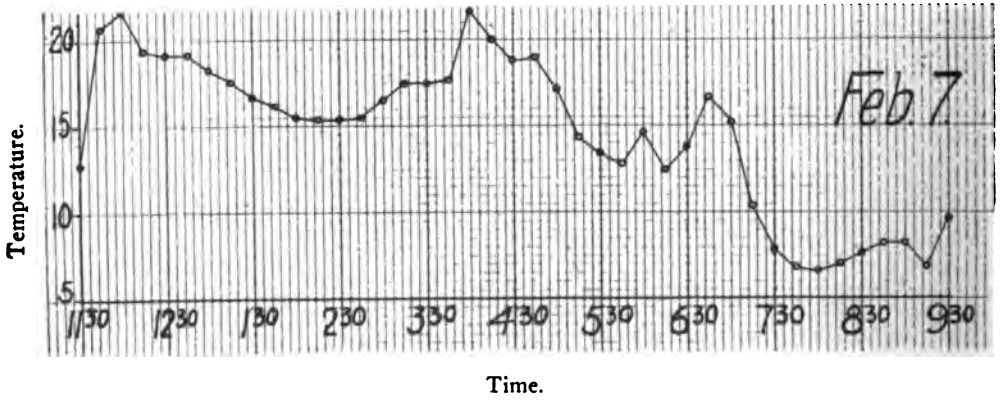
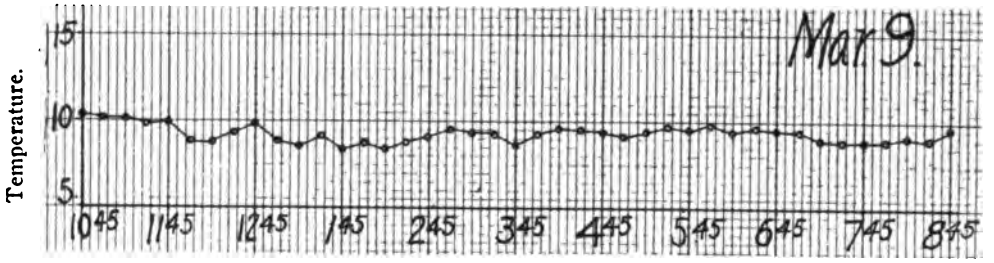


FIGURE 4.—SUCTION TEMPERATURE AT COOLER (CONTINUED).

YORK.

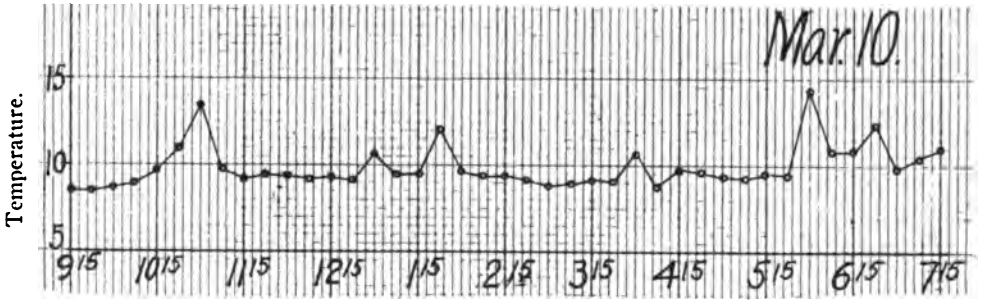
DAY.

4.



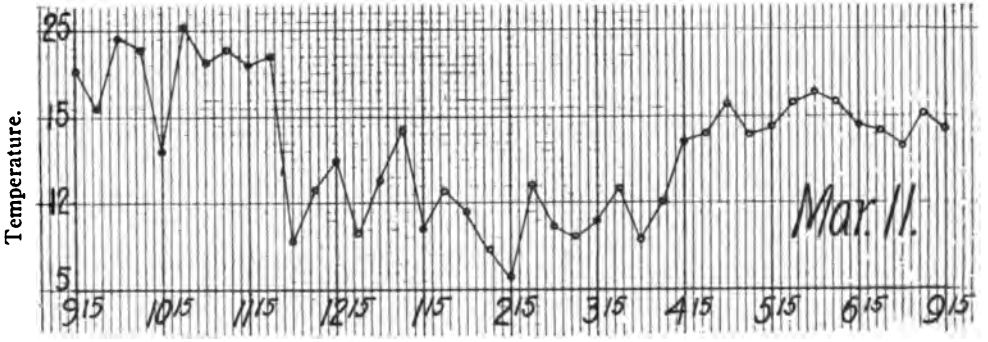
Time.

5.



Time.

6.



Time.

The arrangement of machinery, with some of the measuring apparatus, is shown in the series of photographic illustrations, Figs. 5 to 9. In Fig. 5 the steam engine driven compressor is shown with the steam indicators, as used, attached. The compressor platform and suction piping are shown in Fig. 6, with the De La Vergne compressors in place, and Fig. 7 the York compressors in place. The other end of the engine, showing the frame and main bearings, is illustrated in Fig. 8, which also shows the revolution counter attached to the valve gear and the mercury column for indicating the suction pressure at the compressor. In Fig. 9 are shown the bottom part of the brine cooler and the mercury pressure column, part of the brine pump and brine pipe and the liquid ammonia expansion valve.

The results in horse power per ton for the different days, as completed by the Eastman Kodak Company, are reported in Table XIII. In this table there is not given at any time the actual tonnage, but only an equivalent for 95° liquid, as determined by the Eastman Kodak Company, using the following method:

February 2, 1908.

$$\text{Corrected Tons} = 50.20 \times \frac{\left(\text{Latent heat of vaporization at pressure in cooler}\right) - \left[\left(\text{95° minus temperature at suction pressure}\right) \times \left(\text{Specific heat}\right)\right]}{\left(\text{Latent heat of vaporization at pressure in cooler}\right) - \left\{\left[\left(\text{Actual temperature before expansion valve}\right) - \left(\text{Temperature at suction pressure}\right)\right] \times \left(\text{Specific heat}\right)\right\}}$$

Values interpolated from table in De La Vergne catalogue, page 145:

$$\text{Corrected Tons} = 50.20 \times \frac{551.53 - (95 - 6.45)\,1.1}{551.53 - (76.47 - 6.45)\,1.1}$$

$$= 50.20 \times \frac{454.12}{474.51} = 48.04$$

The method of calculating brine tonnage is as follows:

$$\text{Tons} = \frac{\text{Revolutions of brine pump in fifteen minutes} \times \text{number of pounds of brine per revolution} \times \text{difference of temperature of brine} \times \text{specific heat of brine}}{3000}$$

$$\text{Tons} = \frac{524.3 \times 41.15 \times 10.30 \times .678}{3000} = 50.20$$

FIGURE 5.—THE STEAM-ENGINE-DRIVEN COMPRESSOR, WITH STEAM INDICATORS ATTACHED.

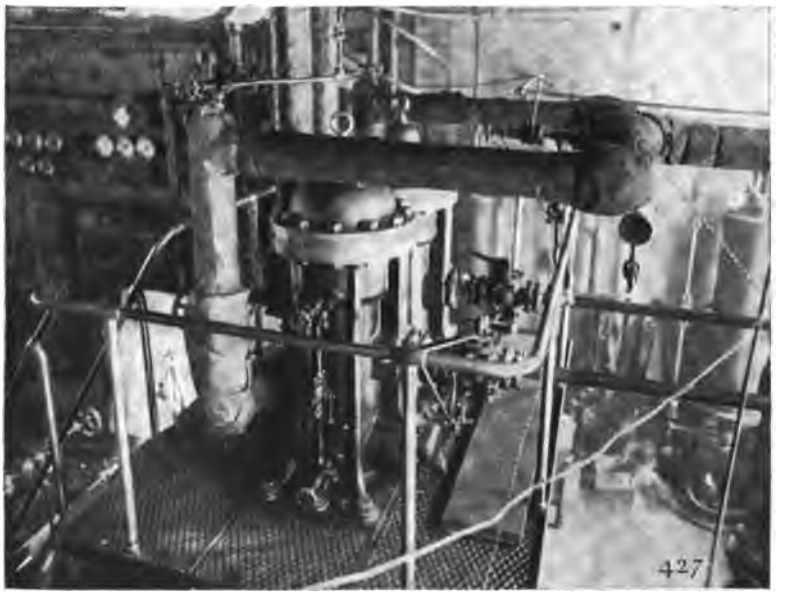


FIGURE 6.—THE COMPRESSOR PLATFORM AND SUCTION PIPING, WITH DE LA VERGNE COMPRESSORS IN PLACE.

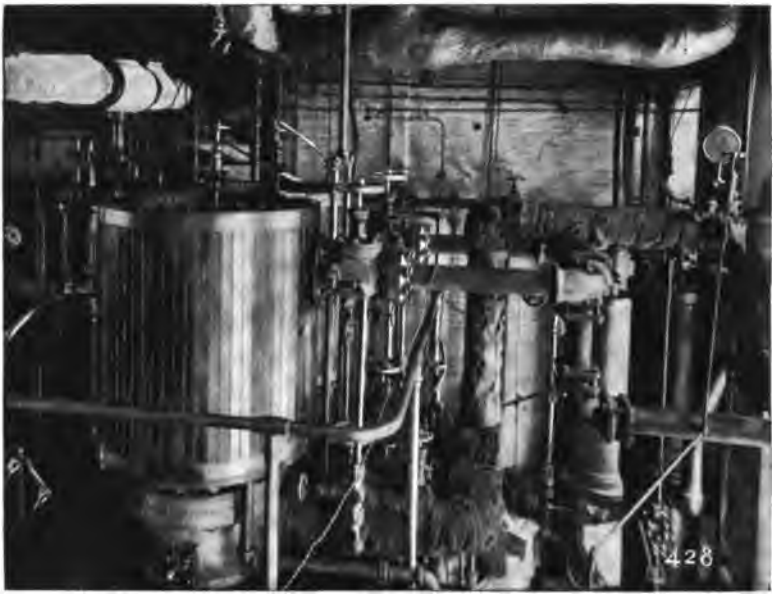


FIGURE 7.—THE YORK COMPRESSORS IN PLACE.

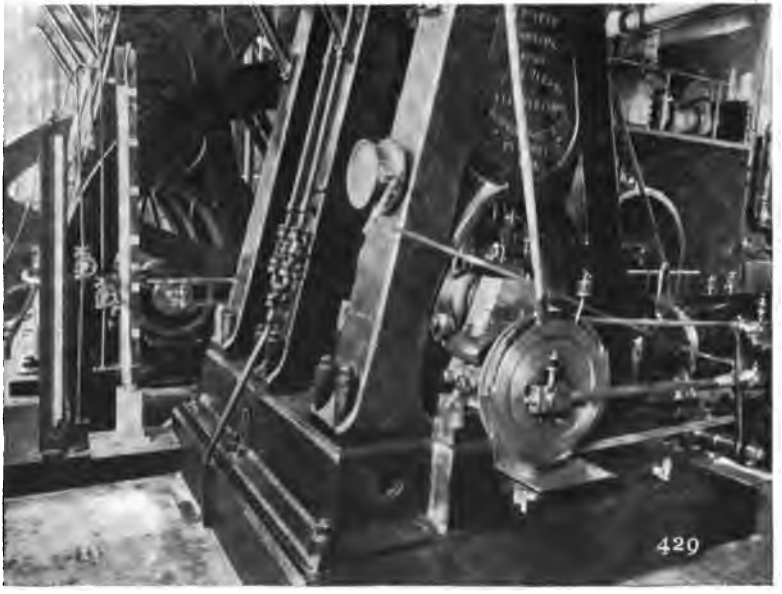


FIGURE 8.—MAIN BEARINGS, SHOWING REVOLUTION-COUNTER AND MERCURY COLUMN.

FIGURE 9.—BOTTOM OF BRINE COOLER AND LIQUID AMMONIA EXPANSION VALVE.

Besides this tonnage value there is a second column reported for the tonnage at 20 pounds pressure, the method of reduction for which is indicated at bottom of Table XIII, proportionately with absolute pressures. While this method of reducing an actual tonnage to the equivalent under some standard conditions is very commonly employed, it is only approximate. The correct method, using these same data, will be employed in the later check calculations. The value of 50.20 as the actual tonnage used above is nearly correct, as will appear from reference to Table XIV, where the actual tonnage is computed step by step for all twelve days.

Tonnage by Brine (Table XIV).

The results reported in this table give the actual tonnage as determined by the brine weight, specific heat and temperature rise under the somewhat variable conditions illustrated by the preceding curves. In order to illustrate the method of procedure, the work for the first day's run, February 2, is presented in full:

$$\overset{\text{Line 1.}}{\left(\begin{matrix}\text{Pounds brine}\\ \text{per pump}\\ \text{revolution}\end{matrix}\right)} 41.5 \times \overset{\text{Line 2.}}{\left(\begin{matrix}\text{Specific}\\ \text{heat of}\\ \text{brine}\end{matrix}\right)} .678 = \overset{\text{Line 3.}}{\left(\begin{matrix}\text{B. T. U. per}\\ \text{pump}\\ \text{revolution}\end{matrix}\right)} 27.8997$$

$$\overset{\text{Line 4.}}{\left(\begin{matrix}\text{Pump revolutions}\\ \text{in fifteen}\\ \text{minutes}\end{matrix}\right)} 524.3 \times 96 = \overset{\text{Line 5.}}{\left(\begin{matrix}\text{Pump revolutions}\\ \text{in twenty-four}\\ \text{hours}\end{matrix}\right)} 50332.8$$

$$27.9 \times 50332.8 = \overset{\text{Line 6.}}{(\text{B. T. U. in twenty-four hours per degree range})} \; 1404270.02$$

$$1404270.02 \times 10.3 \overset{\text{Line 7.}}{(\text{brine temp. range})} = \overset{\text{Line 8.}}{\left(\begin{matrix}\text{B. T. U. per twenty-four}\\ \text{hours from brine}\end{matrix}\right)} \; 14463981.2$$

$$14463981.2 \div 288000 = \overset{\text{Line 9.}}{(\text{Tons refrigeration})} \; 50.22$$

While it is true that this tonnage, based on averages of varying actual conditions, can be reduced to an apparent equivalent for constant standard conditions, yet as this process involves the averaging of a number of observations it is not by any means sure, as explained previously, that the result will really represent what it is supposed to. If the effect of one pound excess suction pressure, for example, is exactly neutralized by a deficiency of one pound for the same time, then the averaging of conditions is justified; but this is not the case. The first step in such a reduction, assuming that the method of averages is justifiable, involves first the determination of how many cubic feet of vapor per hour the compressor drew from the suction pipe.

CUBIC FEET OF VAPOR PER HOUR AT COMPRESSOR (TABLE XV).

The B. T. U. cooling effect per twenty-four hours measured in brine when divided by the available B. T. U. per pound of ammonia as used will give the weight of ammonia per twenty-four hours that must have been used to produce this result in the cooler. This weight of ammonia when it reached the compressor had a certain pressure and temperature, and, therefore, volume per pound, which, when multipled by the weight just mentioned, will give cubic feet of vapor per hour that the compressor must have drawn from the suction pipe. To illustrate the figures which are set down in this table, XV, the following results for the first day, February 2, are abstracted. The liquid temperature, 76.47, line 1; suction temperature at cooler, 21.11° Fahr., line 2, and suction pressure at cooler, 20.64 pounds per square inch above atmosphere, are set down from the averages on the log sheet; but line 4, the barometric pressure, 29.72 inches of mercury, was obtained from the Weather Bureau. This barometer reading is transferred into equivalent pounds per square inch atmospheric pressure, 14.59, line 5, and is added to gauge pressure in the cooler to give the absolute suction pressure at the cooler, 35.23 pounds per square inch, line 6. By using the formula for the temperature of saturated vapor in terms of absolute pressure (Siebel) the absolute temperature of saturated vapor at the cooler pressure, 466.98, line 7, is obtained, and the corresponding vapor temperature, 6.32° Fahr., line 8, follows by subtracting 460.66. The difference between this saturation temperature, 6.32° Fahr., and the actual cooler temperature, 21.11° Fahr., gives the vapor superheat at the cooler, 14.79°, line 9. This vapor superheat, 14.79°, multiplied by the specific heat of vapor, taken as .508, gives the heat of vapor superheat at the cooler in B. T. U. per pound, 7.51, line 11. Similarly the difference between the saturation temperature, 6.32° Fahr., and the liquid temperature at the expansion valve, 76.47°, gives the number of degrees the liquid must be cooled before vaporizing, 70.15, line 10. This liquid superheat multipled by the specific heat of liquid, 1.1, gives the heat of liquid superheat, 77.17 B. T. U., line 12. Interpolation of tables being difficult, the formula for latent heat in terms of saturation temperature (Siebel) was used to find the latent heat, 551.63, line 13. This latent heat is available for cooling both brine and liquid ammonia, as is likewise the heat of vapor superheat, but part of this sum is used in preliminary cooling of the liquid, so that from this sum the heat of liquid superheat must be subtracted to give the B. T. U. available

for brine cooling per pound of ammonia, 481.97, line 14. Multiplying this by twenty-four hours gives the B. T. U. per twenty-four hours, 11,566.8, line 15, for each pound of ammonia used per hour, which divided into the B. T. U. per twenty-four hours given up by the brine, line 16 from Table XIV, gives the pounds of ammonia that must have been used per hour, 1,250.48, line 17. By the formula (Siebel) the cubic feet of saturated vapor per pound are found for the suction pressure at the compressor, 7.991, line 18, and this multiplied by the weight of ammonia vapor gives the cubic feet of saturated vapor per hour at the compressor suction pressure, 9,992.59, line 19. This vapor was, however, superheated, and taking the volumes to be directly proportional to absolute temperature there follows the cubic feet of actual vapor per hour at the compressor, 10,280.4, line 20.

Equivalent Standard Tonnage (Table XVI).

On the assumption that a compressor can take from the suction line the same volume of saturated vapor at any standard absolute pressure as it actually did take of superheated vapor, the equivalent weight of ammonia per hour can be found for the standard reference conditions. Using once more the figures for February 2 to illustrate, there are set down the cubic feet of actual vapor taken per hour by the compressor, 10,280.4, line 1, from Table XV. Dividing this by the volume of standard saturated vapor per pound, 7.95, line 2, found in the same way as before, there results the weight of this standard vapor per hour that the compressor can take, 1,293.1, line 3. For 95° liquid and saturated vapor at the standard absolute pressure the available B. T. U. per pound of ammonia are found to be 453.75, line 5, that would be produced under the standard conditions at the same compressor speed with no pipe-line loss. By the usual constants this is found to represent a tonnage of 48.89 instead of 48.08, as found by the Eastman method.

Real Volumetric Efficiency of Compressors (Table XVII).

In judging the results of tonnage produced one of the most instructive bases of comparison is the true volumetric efficiency of the compressor. This efficiency is the ratio of the actual volume of vapor drawn from the suction pipe to the compressor displacement for the same time. In this table the volume of vapor per hour, 10,280.4, line 1, from the suction is set down from Table XV, the

compressor diameter, 11¼ inches, line 2, and stroke, 22 inches, line 3. From these compressor dimensions the displacement per revolution, 4.937, line 4, for the two double-acting compressors is computed, making due allowances for the piston rod, and this multiplied by the average revolutions in fifteen minutes, 676.7, line 5, from the log sheet, and by four gives the displacement in cubic feet per hour, 13,363.5, line 7. Dividing the volume of suction vapor handled, line 1, by this gives the volumetric efficiency, 76.93, line 8. It appears from the volume found that for the first three days when the speed was about 45 R. P. M., at which the machine was rated, the De La Vergne compressor showed a little over 77½ per cent. volumetric efficiency, and that this value decreased by suction valve throttling on the highest speed run, about 67 R. P. M., to almost 75½ per cent., as might be expected. The facts that at the intermediate speed, about 56 R. P. M., the volumetric efficiency had not likewise an intermediate value, but was actually lower than for the maximum speed, and also that for the lowest speed, about 34 R. P. M., the value, 76.81, was not higher than for 45 R. P. M., but lower, throw the first doubt on the accuracy of the data. This discrepancy also appears in the York results, where the volumetric efficiencies placed in the order of increasing speeds and averaging the first three days are 84.51 per cent., 87.05 per cent., 87.12 per cent., 85.31 per cent. Instead of a decrease with increase of speed there is for three speeds an actual increase. These real volumetric efficiency values are, moreover, extraordinarily high, and so far as the writer is aware considerably higher than ever claimed for any compressor. In this connection the old familiar tests reported by Professors Jacobus and Denton in the introduction to the little book by Ledoux furnish some data of value in comparison. On page 30 of the book it is stated that the loss by cylinder heating alone was between 15 per cent. and 30 per cent., and as the above volumetric efficiency differs from 100 per cent. by reason of both cylinder heating and re-expansion, the results of the York test are quite good, all being higher than the highest quoted. In fact, they are so very good as to make it incumbent on the builders of the machine to explain just what features of construction were incorporated in this compressor to produce them. The figures appear even more remarkable when the cylinder heating effect on suction that is involved in them is separately examined.

TABLE XVII.

REAL VOLUMETRIC EFFICIENCY.

De La Vergne Machine.

Date	*Feb. 2.*	*Feb. 3.*	*Feb. 4.*	*Feb. 5.*	*Feb. 6.*	*Feb. 7.*
1. Cu. Ft. Vapor Per Hour at Comp...	10280.4	10447.7	10341.3	7768.3	12228.8	15118.2
2. Diameter, Inches..	11.25	11.25	11.25	11.25	11.25	11.25
3. Stroke, Inches.....	22	22	22	22	22	22
4. Cu. Ft. Per Rev...	4.937	4.937	4.937	4.937	4.937	4.937
5. Rev. in 15 Min....	676.7	674.7	675.0	512.1	842.0	1015.2
6. Rev. Per Hour....	2706.8	2698.8	2700.0	2048.4	3368.0	4061.2
7. Cu. Ft. Per Hr. Displacement	13363.5	13324.0	13330.0	10113.0	16627.8	20050.1
8. Vol. Eff., Per Cent.	76.93	78.41	77.58	76.81	73.54	75.40

York Machine.

Date	*Mar. 6.*	*Mar. 7.*	*Mar. 8.*	*Mar. 9.*	*Mar. 10.*	*Mar. 11.*
1. Cu Ft. Vapor Per Hour at Comp...	10455.1	10456.4	10900.4	7830.7	13176.8	15619.6
2. Diameter, Inches..	15	15	15	15	15	15
3. Stroke, Inches....	22	22	22	22	22	22
4. Cu. Ft. Per Rev...	4.5006	4.5006	4.5006	4.5006	4.5006	4.5006
5. Rev. in 15 Min...	677.2	675.75	677.1	514.7	840.2	1017.05
6. Rev. Per Hour....	2708.8	2703.0	2708.4	2058.8	3360.8	4068.2
7. Cu. Ft. Per Hr. Displacement ...	12191.2	12165.1	12189.4	9265.8	15125.6	18309.3
8. Vol. Eff., Per Cent.	85.76	85.96	89.42	84.51	87.12	85.31

Cylinder Heating Effect (Table XVIII).

From the indicator card the point of the stroke at which the re-expansion line crosses the suction line permits of the measurement of the remainder, which is effective in suction. The ratio of the length of the effective suction line to the stroke is the apparent volumetric efficiency. The length of the stroke during which the suction valve is open, multiplied by the area of the piston, gives the volume of gas apparently drawn in, while the amount really drawn in has been found from the tonnage. The ratio of these volumes gives the ratio of the absolute temperatures before and after suction, or the ratio of suction pipe temperature to temperature of gas in the cylinder after suction is complete and the valve closed. Obviously this comparison cannot be made with liquid injection of unknown amounts into the suction pipe, and for that reason results are reported for the York machine only. The apparent volumetric efficiency taken from the York ammonia cards averaged about 98

per cent., and no differences could be definitely fixed for different cards, because of the thickness of the lines and the smallness of the distances; this value has, therefore, been taken as most probably correct for all runs. This is equivalent to only 2 per cent. re-expansion, which, by computation, appears to be equivalent to a clearance of less than 1 per cent., about .7 per cent. If the cylinder and piston were both truly round and of uniform diameter at the top, and the piston and cylinder heads truly flat, then the linear clearance corresponding to this volume would be about one-seventh of an inch, an extremely small value. If, however, as is probably the case, there is some space between the piston up to the first ring and the cylinder, when in the top position, and if at the same time neither cylinder head nor piston head, both of which carry valves, be not perfectly flat, then the linear clearance will be something less than one-seventh of an inch, and probably not over half of this. Such small clearances seem to be hardly safe, and the best air compressor practice would not permit such close working. The best air compressors, even of large size and using Corliss valves, are not considered safe with less than 2 per cent. to 3 per cent. clearance. However, the fact remains that this compressor did operate without accident during the test; but what would happen with this clearance when considerable quantities of liquid came over from the cylinder, or with solid matter on the piston, such as broken valves, remains an open question. It has always been contended, and with justice, by the De La Vergne Machine Company that safety in handling cylinder liquid demanded clearance in which it could do no harm, and safety from broken valves demanded that they be located in pockets, which, of course, limited their size. The suction valve resistance work, amounting in these tests to between 2 per cent. and 6 per cent. of the work of compression in different runs, is not a high price to pay for the safety element resulting. Referring to the table (No. XVIII) for the first day, March 6, the apparent volumetric efficiency, .98, line 1, is taken from the indicator cards, while the real volumetric efficiency, .858, line 2, is taken from Table XVII. The ratio of the former to the latter, 1.142, line 3, is likewise the ratio of the absolute temperatures, which, applied to the temperature before suction, 15.15° Fahr., or 475.81° absolute, line 4, gives the temperature after suction, 543.37° absolute, or 82.71° Fahr., line 5. Subtracting from this the temperature before suction, there results the temperature rise on entrance to the cylinder, 67.56°, line 6. It is interesting to compare these temperature increases, which range from about 49° to 77°, with the jacket and discharge temperatures. On the fourth day, that of

maximum gas heating, the jacket temperature was about equal to that of the third day, when the gas heating was least, and an increase of over 100° in jacket temperature, second to third day, corresponded to a decrease of cylinder heating effect, proving that either the data are misleading, or that water jackets have no effect on the temperature of suction gas entering the cylinder. Comparing the jacket temperature with the discharge gas temperature, it likewise appears that there is no direct relation between them, because with the change of jacket temperature of about 100° there is not over 2° difference in discharge gas on the fourth and sixth days. When, however, the fourth and fifth days are compared there is found a difference of some 7° in discharge gas, with no change in jacket temperature. These facts seem to prove what is now accepted in air compressor work, that the jacket has practically no effect on the gas in compression.

TABLE XVIII.

SUCTION HEATING EFFECT.

York Machine.

Date	*Mar. 6.*	*Mar. 7.*	*Mar. 8.*	*Mar. 9.*	*Mar. 10.*	*Mar. 11.*
1. Apparent Vol. Eff.	.98	.98	.98	.98	.98	.98
2. Real Vol. Eff.	.858	.859	.894	.845	.871	.853
3. Ratio $\frac{\text{No. 1.}}{\text{No. 2.}}$	1.142	1.141	1.095	1.159	1.125	1.149
4. Temp. Before Suction	15.15 475.81	14.28 474.94	16.79 477.45	14.77 475.43	13.52 474.18	18.15 478.81
5. Temp. After Suction	543.37 82.71	541.91 81.25	522.81 62.15	551.02 90.36	533.45 72.79	550.15 89.49
6. Temp. Rise During Suction	67.56	66.97	45.36	75.59	59.27	71.34
7. Jacket Temp.	....	73.3	176.3	174.7	174.7	75.05
8. Discharge Gas Temp.	216.25	217.78	250.52	245.8	252.95	242.9

Taking into consideration the temperature of the gas in the compressor, after suction is completed, as calculated in Table XVIII, it can be figured that the gas in the cylinder, after compression is completed, is considerably hotter than the discharge gas, which on the last four days averaged, roughly, 250°. It is likewise true that at the top of the cylinder the metal must have a temperature between that of the discharge gas and the water, and at the moment of discharge it is likely that the skin is as hot as the discharge gas, while the discharge valve probably remains as hot practically all the time. The piston head being likewise uncooled, except as it gives heat to entering gas, and so heats it beforehand, it would

seem reasonable to expect a material heating of the gas entering through the narrow slit orifice of the piston valve and impinging on hot walls, cylinder head and discharge valve. It would seem not unreasonable to expect the gas to at least attain a temperature midway between discharge and suction gas, which would require something over a 100° rise, nearly twice as much as found. The temperature rise of the gas before compression based on such an analysis seems unaccountably low. Jacobus and Denton from the tests previously mentioned give on page 54 the following:

Suction Pressure, Pounds Absolute	22.9	27.7
Suction Temperature, Fahrenheit	25°	10°
Condenser Pressure, Pounds Absolute	167	162
Temperature of Gas at End of Suction	157.7°	127°

These heating effects are all much higher than computed for the York tests, although the conditions of pressure are quite commensurate. If the York results are correct the performance is quite remarkable; but if, on the contrary, the gas was actually heated more, then the compressor did not handle the quantity of gas or produce the tonnage reported, again throwing some doubt on the consistency and accuracy of the data.

Comparison of Indicator Cards.

The first step in analyzing the probable accuracy of the indicated horse power, and in explaining the differences, involves a judgment of the value of the indicator cards from which the mean effective pressure for the steam cylinder is found. It is a well-known fact that the M. E. P., as determined from an indicator card, is a somewhat uncertain thing, involving as it does changes in the cord stretch, errors of pressure spring, errors in measuring areas and lengths, as well as the more important error of the true average. When, as in these tests, one card or set taken at fifteen-minute intervals is assumed to represent a true average of all those produced between, considerable discrepancies may creep in. At 45 R. P. M. there would be produced 45 head end cards per minute, or $15 \times 45 = 675$ head end cards in fifteen minutes, so that the slightest accident at the time of taking may result in a wrong area, which, when assumed to last for the whole, as was done, really multiplies the error by 675 in its effect. To permit of judging of the existence of such accidents there are two methods open, both of which have been used. First, as the setting of the valve gear was unchanged for the entire series, there ought to be a consistent difference between head end and crank end areas, and any inconsistency is a measure of the limit of accuracy of the results, or an indication that the card taken

did not represent a true average of 675 total steam admissions. Secondly, whenever the suction and condenser pressures were the same for any instant, then the machine should produce the same steam card, and the areas of several such should be identical. Departures from identity indicate the same thing as inconsistency in the difference between head and crank end cards.

First Method.

All the card areas reported were examined and the differences between head end and crank end found, and of these the maximum and minimum are set down for each day in Table XIX. In both series the minimum difference is about zero, but more consistently

TABLE XIX.

DIFFERENCE IN SQUARE INCHES BETWEEN AREAS OF HEAD AND CRANK END STEAM INDICATOR CARDS.

De La Vergne Machine.

	MAXIMUM.			*MINIMUM.*		
Day.	*Head End.*	*Crank End.*	*Difference.*	*Head End.*	*Crank End.*	*Difference.*
February 2	2.33	—2.21	= +.12	2.07	—2.08	= —.01
" 2	2.37	—2.25	= +.12	.....		
" 3	2.37	—2.20	= +.17	2.22	—2.21	= +.01
" 4	2.37	—2.22	= +.15	2.32	—2.315	= +.005
" 5	2.195	—2.25	= —.055	2.24	—2.24	=0
" 5	.....			2.28	—2.28	=0
" 6	2.41	—2.28	= +.13	2.33	—2.33	=0
" 6	.....			2.38	—2.38	=0
" 7	2.37	—2.21	= +.16	2.32	—2.32	=0

York Machine.

Day.	*Head End.*	*Crank End.*	*Difference.*	*Head End.*	*Crank End.*	*Difference.*
March 6	2.06	—2.16	= —.10	2.13	—2.13	=0
" 7	2.175	—2.12	= +.055	2.11	—2.11	=0
" 7	.....			2.14	—2.14	=0
" 8	2.155	—2.10	= +.055	2.12	—2.12	=0
" 9	2.10	—2.16	= —.06	2.075	—2.075	=0
" 9	.....			2.15	—2.15	=0
" 10	2.17	—2.26	= —.09	2.17	—2.17	=0
" 10	.....			2.21	—2.21	=0
" 11	2.14	—2.06	= +.08	2.13	—2.13	=0

so for the York than for the De La Vergne series; this means that at some time of every day the head and crank end cards had equal areas, even though the areas varied from 2.07 to 2.38 square inches on the head end. For the same days, while the head end cards were between 2.06 and 2.41 square inches, there were very considerable fluctuations between head and crank end areas, reaching

as high as .17 square inch on February 4 for the De La Vergne, and consistently high for this end, except for one day, February 5, when it fell to about zero. The York series is, however, quite inconsistent in this maximum difference, the head end exceeding the crank by only .08, on March 11, less than half the excess for the De La Vergne, and reversing on March 6, so that the crank end exceeded the head by .10. This swing from + .8 square inch to — .10 square inch represents a discrepancy of .18 square inch, which is 8.5 per cent. of the mean area of four cards, 2.105 square inches, while at other times cards ranging from about the same to greater loads showed equality between head and crank ends. Similarly, examining the De La Vergne series, the above per cent., which may be considered a measure of unreliability, reaches 10 per cent. Another way of looking at the question involves a comparison of averages. The mean of all the maximum differences for the De La Vergne series is + .091 square inch, while for the York it is — .02 square inch, while the same for the minima is + .005 for the De La Vergne and 0 for the York. Such discrepancies as these are evidence of either a most erratic valve gear or carelessness or accident in getting cards to represent a true average. There seems to be no other explanation of the fact that while at some time of every run the head and crank cards are equal, yet at the same loads at other times the De La Vergne head end cards are consistently larger, reaching .17 square inch excess, and the York cards swing from less than half of this excess on the head end to .10 square inch in the other direction.

Second Method.

All the suction and condenser pressures were set down in the order of magnitude, and card areas set opposite so that the card areas might be compared for identical suction and condenser pressures. Both the York and De La Vergne machines are thus found at different times to give identical card areas, but at others the same sort of discrepancy previously noted in the comparison of head end and crank end cards is found. The following table, giving one case for each machine, shows how perfectly the identity which should exist actually did exist:

	De La Vergne Machine.		York Machine.	
Date	*Feb. 5.*	*Feb. 5.*	*Mar. 8.*	*Mar. 8.*
Suction Pressure, Pounds Absolute	35.51	35.51	35.12	35.12
Ratio $\frac{\text{Condenser Pressure, Lbs. Absol.}}{\text{Suction Pressure, Lbs. Absol.}}$	5.69	5.69	5.73	5.73
Time	3.15	5.30	11.45	7.15
Head End Steam Area, Square Inches	2.28	2.28	2.14	2.14
Crank End Steam Area, Square Inches	2.28	2.27	2.12	2.15

From this it appears that when at 5:30 P. M., February 5, the same compressor conditions were reproduced as existed at 3:15 P. M. on the same day, the same indicator card areas to within less than 1 per cent. were reproduced, and the same thing happened for the York machine on March 8. This indicates that for part of the time at least correct cards were obtained, and that they were correctly measured for area. On the contrary, however, there exists a considerable difference at other times, and of nearly the same magnitude as found by the first method of comparison, and indicating unreliability of card areas for the same periods between 5 per cent. and 10 per cent. Finally, comparing the card areas from the De La Vergne machine with those from the York machine for identical suction and condenser pressures, there will be found in general a fairly constant ratio.

COMPARISON OF DE LA VERGNE AND YORK CARDS FOR SAME COMPRESSOR CONDITIONS.

Abs. Suct. Press. *Pounds.*	*Ratio Cond. Pr. / Suct. Pr.*	*Day.*	*Time.*	*Head. Area. Sq. In.*	*Crank Area. Sq. In*	*Machine.*
35.11	5.73	Feb. 4	6.45	2.30	2.26	De La Vergne.
35.11	5.73	Mar. 10	9.45	2.18	2.20	York.
				.12	.06	Difference.
35.24	5.59	Feb. 2	6.00	2.31	2.22	De La Vergne.
35.24	5.59	Mar. 7	2.45	2.105	2.15	York.
				.205	.07	Difference.
35.26	5.67	Feb. 5	3.00	2.25	2.24	De La Vergne.
35.26	5.67	Mar. 7	1.45	2.14	2.14	York.
				.11	.10	Difference.
35 44	5.60	Feb. 2	3.45	2.28	2.23	De La Vergne.
		Mar. 6	12.45	2.15	2.115	York.
				.13	.115	Difference.

It will be seen from the above that the De La Vergne machine always produces a greater steam card area than the York for the same suction and condenser pressures, and adding head and crank areas for each machine it appears that the De La Vergne area is from 1.04 to 1.06 times that of the York machine. This is extremely interesting when compared with the suction valve resistances of the two machines from their indicator cards. The York cards showed

no perceptible drop, while the De La Vergne cards showed a resistance loss of about 6 per cent. at the overload speed of 67 R. P. M., and about 3 per cent. at the rated speed of 45 R. P. M., the loss varying with the square of the speed. This suction valve resistance loss, then, just accounts for the excess of steam card area found for the De La Vergne machine over the York, and, as previously pointed out, is the price of the safety of the De La Vergne construction.

Equivalent Horse Power With Standard Conditions (Table XX).

Assuming the steam card areas to be correct as reported, which, by the preceding discussion, is shown to be not so, the horse power resulting from the actual suction and condenser pressures is reduced in Table XX to the equivalent for the standard reference suction and condenser pressures. For this reduction it has been assumed that the ratio of the two mean effective pressures is given by the following equation, where r = the ratio of absolute condenser to absolute suction pressure:

$$\frac{\text{M. E. P. Standard}}{\text{M. E. P. Actual}} = \frac{34.7\,[(5.75)^{.23} - 1]}{\text{Abs. Suct. Pr.}\,[(r)^{.23} - 1]} = 1.008 \text{ for February 2.}$$

From the table it appears that this reduction of horse power to standard conditions resulted in a change which was in general less than 1 per cent., but reached in one case, that of March 11, 2½ per cent.

TABLE XX.

REDUCTION OF HORSE POWER TO 20 POUNDS SUCTION AND 185 POUNDS CONDENSER PRESSURES WITH STANDARD BAUMÉ ON ASSUMED CORRECT DATA.

De La Vergne Machine.

Date	*Feb. 2.*	*Feb. 3.*	*Feb. 4.*	*Feb. 5.*	*Feb. 6.*	*Feb. 7.*
1. Abs. Suct. Pr.	34.56	34.64	35.00	35.04	34.55	33.25
2. Abs. Cond. Pr.	197.89	200.73	200.58	200.07	200.43	200.07
3. Ratio $\frac{\text{Abs. Cond. Pr. Actual.}}{\text{Abs. Suct. Pr. Actual.}}$	5.72	5.79	5.73	5.79	5.8	6.02
4. Abs. Suct. Pr. for 14.7 Standard and 20 lbs.	34.70	34.70	34.70	34.70	34.70	34.70
5. Abs. Suct. Pr. for 14.7 Standard and 185 lbs.	199.7	199.7	199.7	199.7	199.7	199.7

TABLE XX—(Continued).

REDUCTION OF HORSE POWER TO 20 POUNDS SUCTION AND 185 POUNDS CONDENSER PRESSURES WITH STANDARD BAUMÉ ON ASSUMED CORRECT DATA.

De La Vergne Machine.

Date.....	*Feb. 2.*	*Feb. 3.*	*Feb. 4.*	*Feb. 5.*	*Feb. 6.*	*Feb. 7.*
6. Ratio $\frac{\text{Abs. Cond. Pr. Standard.}}{\text{Abs. Suct. Pr. Standard.}}$	5.75	5.75	5.75	5.75	5.75	5.75
7. Ratio $\frac{\text{M. E. P. Standard.}}{\text{M. E. P. Actual.}}$	1.006	.996	.994	.985	.998	1.013
8. H. P. Reported.....	74.06	74.55	75.41	55.83	94.95	114.13
9. I. H. P. for Standard	74.50	74.25	74.96	54.99	94.76	115.39

York Machine.

Date.....	*Mar. 6.*	*Mar. 7.*	*Mar. 8.*	*Mar. 9.*	*Mar. 10.*	*Mar. 11.*
1. Abs. Suct. Pr.....	34.79	34.52	34.35	34.99	34.26	31.76
2. Abs. Cond. Pr.....	198.64	199.03	201.37	202.22	202.93	201.59
3. Ratio $\frac{\text{Abs. Cond. Pr. Actual.}}{\text{Abs. Suct. Pr. Actual.}}$	5.71	5.76	5.86	5.78	5.92	6.34
4. Abs. Suct. Pr. for 14.7 Standard and 20 lbs.....	34.70	34.70	34.70	34.70	34.70	34.70
5. Abs. Suct. Pr. for 14.7 Standard and 185 lbs.....	199.7	199.7	199.7	199.7	199.7	199.7
6. Ratio $\frac{\text{Abs. Cond. Pr. Standard.}}{\text{Abs. Suct. Pr. Standard.}}$	5.75	5.75	5.75	5.75	5.75	5.75
7. Ratio $\frac{\text{M. E. P. Standard.}}{\text{M. E. P. Actual.}}$	1.002	1.004	.997	.987	.992	1.023
8. H. P. Reported.....	69.35	69.80	70.05	52.57	89.48	105.11
9. I. H. P. for Standard	69.49	70.08	69.84	51.89	88.76	107.53

Comparison of Results (Table XXI).

There are calculated in Table 21 the horse power and tonnages as reported, and as reduced to standard conditions from assumed correct data for the purposes of comparison, and it seems that the excess of performance of the York machine is still further increased

when reduced to standard conditions, which fact further accounts for the unreliability of some of the data, if that were necessary.

TABLE XXI.

COMPARISON OF STEAM I. H. P. PER TON AS REPORTED AND REDUCED TO STANDARD CONDITIONS ON ASSUMED CORRECT DATA.

	First Day.		Second Day.	
	As Reported.	*Reduced.*	*As Reported.*	*Reduced.*
Tonnage, D. L. V.	48.08	48.89	48.94	49.69
Tonnage, York	48.92	49.73	48.93	49.74
I. H. P., Steam, D. L. V.	74.06	74.50	74.55	74.25
I. H. P., Steam, York	69.35	69.49	69.80	70.08
Steam I. H. P. Per Ton D. L. V.	1.540	1.524	1.523	1.494
Steam I. H. P. Per Ton York	1.418	1.399	1.427	1.409
Ratio $\frac{\text{D. L. V. Valve}}{\text{York Valve}}$	1.086	1.090	1.067	1.061

	Third Day.		Average of First Three Days.		Fourth Day.	
	As Reported.	*Reduced.*	*As Reported.*	*Reduced.*	*As Reported.*	*Reduced.*
Tonnage, D. L. V.	49.18	49.19			36.88	36.95
Tonnage, York	50.42	51.84			36.97	37.25
I. H. P., Steam, D. L. V.	75.41	74.96			55.83	54.99
I. H. P. Steam, York	70.05	69.84			52.57	51.89
Steam I. H. P. Per Ton D. L. V.	1.533	1.524	1.531	1.514	1.514	1.488
Steam I. H. P. Per Ton York	1.389	1.347	1.411	1.385	1.422	1.393
Ratio $\frac{\text{D. L. V. Valve}}{\text{York Valve}}$	1.103	1.133	1.085	1.095	1.064	1.068

	Fifth Day.		Sixth Day.	
	As Reported.	*Reduced.*	*As Reported.*	*Reduced.*
Tonnage, D. L. V.	57.63	58.16	71.14	71.91
Tonnage, York	62.78	62.67	73.03	74.29
I. H. P., Steam, D. L. V.	94.95	94.76	114.13	115.39
I. H. P., Steam, York	89.48	88.76	105.11	107.53
Steam I. H. P. Per Ton D. L. V.	1.648	1.629	1.604	1.604
Steam I. H. P. Per Ton York	1.425	1.416	1.439	1.447
Ratio $\frac{\text{D. L. V. Valve}}{\text{York Valve}}$	1.156	1.151	1.114	1.110

Conclusions.

The conclusions that can be drawn from this analysis of the Rochester test can be divided into positive and negative.

First.—The tests do not demonstrate the superiority of a single-acting over a double-acting compressor, of a York over a De La

Vergne compressor, or dry compression over wet compression, in spite of the fact that the horse power per ton reported is less for the York than for the De La Vergne machine; but simply show that small valve areas require more work than large in the driving cylinder.

Second.—The tonnage reported is probably too large for the York tests, as shown by the extremely small temperature rises during suction and high real volumetric efficiencies.

Third.—The horse power reported is not correct within 10 per cent., and the probabilities are that the horse power reported for the De La Vergne series is more in excess of the true value than for the York series, or that correct values for the horse power would reduce that required by the De La Vergne machine more than for the York machine, and render horse power per ton more nearly equal.

Fourth.—Water jackets have practically no effect on the performance of the compressor.

Fifth.—Clearance of the York compressor in the test was unusually small, and may fairly be considered too small for reliable commercial operation.

Sixth.—Even if the data were all correct and reliable, the comparison of the two machines on the basis of results would be unfair and misleading. Any conclusions on the relative superiority are impossible, because of the inequality of conditions under which the machines operated, as shown by the plotted curves.

Seventh.—It is impossible with water-jacketed compressors to discharge gas as cool as can be maintained within internally cooled compressors using oil and liquid ammonia.

Eighth.—There is no information on the precise value of liquid injection.

Ninth.—There is practically no difference between the performance of these two different types of compressors in horse power per ton, except as may be traced to difference in suction valve resistance.

The relative values of the compressors are not to be determined by the horse power per ton, even if the correct values were known for each, which is not the case here, but by structural qualities involving reliability, life, cost of maintenance, safety to property and safety to life. Even if one compressor actually did have a little better horse power per ton than the other, it would affect the coal consumption per ton only through two other quantities, the steam consumption per horse power and the pounds of steam evaporated

per pound of coal, both of which are subject to far greater differences of accidental nature than the horse power per ton. A poorly constructed engine that soon becomes leaky, or a boiler operated carelessly, may separately or together result in changes in the coal consumption per steam horse power several times as great as the horse power per ton. It is very significant that the De La Vergne compressor used in these tests gave after seventeen years' continuous service results quite as good as when first installed, and results which, compared with the new York compressors, are almost identical, much more nearly so than the figures reported indicate, as shown by the analysis, and the query "Is the York compressor so constituted as to make it capable of reproducing its best results after an equally long, safe and economical service?" becomes decidedly pertinent.

In conclusion it may be said concerning these elaborate tests, which at first glance seem to bear on the question of the relative value of the machines, and on the assignment of the proper reasons, that nothing applies so well as the famous Scotch verdict of "nothing proved."

DISCUSSION.

Thomas Shipley.—Professor Lucke's paper is interesting to all of us. It shows that he prepared it with great care, and he deserves credit for his efforts. It is my opinion that his paper will save some men from making bad investments, and it will certainly lead many to improve the bad investments they have already made.

There are some statements Professor Lucke has made that I would like to discuss, and in addition there are some questions I wish to ask him, but before I do so I believe it would be well to place upon our records the facts relating to the conditions which brought about the Eastman Kodak Company's tests, stating the object of the tests, the agreement under which they were made, and who represented the different parties. This will make the records of our Society more complete and enable men who are not familiar with the facts to see that the tests were no mere accident—that there was sufficient value at stake for each of the parties involved to make everyone do his best, and further, that the men who conducted the tests were fitted in every way to give a good account of themselves in the work. In fact, no such tests have ever been made before. At least, there was never a test made before where the understanding was to make the data obtained public property, no matter whom it hurt; hence I believe it the proper thing to place the following facts upon our records.

The Eastman Kodak Company, of Rochester, N. Y., desiring to make a large increase to its refrigerating plant, investigated the different types of machinery built for this work, and while its engineers were engaged in this investigation certain statements were made to them as to the relative efficiency of double-acting and single-acting ammonia compressors. After getting what data they could upon the subject, they concluded that it would be to the advantage of their company to go to the expense of having a test made at the Kodak Park Works, at Rochester, to determine to their satisfaction whether or not there was a difference between these types of compressors, and what it was.

The Eastman Kodak Company had several double-acting machines of the De La Vergne Machine Company's make at its Kodak Park Works, and under the advice of its engineers it offered the use of one of them to the De La Vergne Machine Company, of New York, and to the York Manufacturing Company, of York, Pa., that they might make a test to determine the question as to whether the double-acting compressor or the single-acting compressor required more horse power per ton of refrigeration, and from the results of this test the Eastman Kodak Company was to decide to which company it would give the contract for two 400-ton cross-compound refrigerating machines with high pressure sides complete.

The Eastman Kodak Company could not afford to make a mistake, for the plant it was about to install was a very large one and a mistake would be costly; hence it wanted the very best machinery available.

The De La Vergne Machine Company could not afford to fail in making good its claims as to the superior efficiency of its machinery, for by so doing it would lose not only a very large contract and a good customer but in addition its business would suffer materially.

The York Manufacturing Company realized that the opportunity which it had been striving to obtain for many years was at hand, and if it failed to establish its claims, namely, that its single-acting compressor was more efficient than De La Vergne compressors, it would not only lose a very large contract but its business would get a very decided setback.

All three of the parties, realizing the seriousness of the trial, proceeded with all the care at their command, with the result that the following agreement was entered into on January 4, 1908:

COPY OF AGREEMENT.

First.—The parties to these tests shall be as follows:

The Eastman Kodak Company, of Rochester, N. Y., party of first part;

The De La Vergne Machine Company, of New York, N. Y., party of the second part; and

The York Manufacturing Company, of York, Pa., party of the third part.

Second.—The parties hereto hereby agree to test the machine known as the 50-ton machine of the make of the party of the second part owned by the party of the first part and now in operation at their Kodak Park Works.

Third.—The object of these two series of tests is to determine, first, the difference in steam I. H. P. per ton of refrigeration at 45 R. P. M. of this machine, operating with two 11x22-inch double-acting oil compressors of the party of the second part's make, or whatever size the present cylinder may be, and its steam I. H. P. per ton of refrigeration at 45 R. P. M. when operating with two 15x22-inch single-acting compressors of the party of the third part's make, and, second, their capacity and steam I. H. P. at other speeds; the tests to be conducted as follows:

The machine to be first tested with the 11x22-inch double-acting oil compressors, or whatever size the present cylinders may be, this test to be under the direction of the party of the second part, they having the privilege to put the compressors and connections thereto in what they consider first-class working condition, and if, in their opinion, it is necessary to make any changes to the compressors or connections, they are to be allowed to do so.

The second test of the machine to be with the 15x22-inch single-acting compressors, this test to be under the direction of the party of the third part, they having the privilege to put the compressors and connections thereto in what they consider first-class working condition, and if, in their opinion, it is necessary to make any changes to the compressors or connections, they are to be allowed to do so, but the engine is to remain as it is for the first test.

There are to be two series of tests with each type of compressors, each series to consist of three tests, and each test to be of ten hours' duration, the machine to be run not more than three hours before the beginning of each test, it being understood that only one test shall be made in twenty-four hours, each series of tests to be under the conditions hereafter stated.

The first series of tests shall be at 45 revolutions at 20 pounds suction pressure at the machine and with 185 pounds high pressure.

The second series of tests shall be a capacity test of the machine, the machine to be run at the same pressures as in the first series, but at whatever speed the parties of the second and third parts, whoever may be conducting the test, may deem the proper working speed of the machine suitable to the type of compressors under test.

It is the purpose of all the parties hereto to begin the tests at the earliest possible date, and unless serious and unforeseen accident shall prevent, the tests shall be run on six consecutive days.

It is also understood that the condensers now installed above the engine room in which the machine to be tested is located shall be used for the purpose of these tests.

It is understood that the suction pressure is to be determined by a mercury column, other pressures to be determined by proper gauges; that the temperatures of the different parts of the apparatus needed to complete the data are to be determined by the thermometers inserted in proper wells fitted or attached to the apparatus whenever same is necessary; that the amount of brine is to be determined by the calibration of the pump used, at the beginning of each test and at the end of each test, if desired by the party conducting the test. It is understood that the temperature of the outgoing brine is to be as nearly the same in all tests as possible, and that if it is necessary the parties of the second and third parts will install to their mutual satisfaction the necessary apparatus to heat the brine to the temperature needed for the purpose of the test.

It is also understood between the parties hereto that the tests shall be conducted similar to the tests made by the Ice Machine Builders' Association at the York Manufacturing Company's plant in York, Pa., in October, 1903;

that there will be a log board provided, properly ruled, with the proper headings, upon which shall be entered all readings taken during the intervals of the tests, that the parties hereto shall have the privilege of objecting to the correctness of any of the readings when same are placed on the board. If, however, no objection is made to the readings during the test the same shall be considered acceptable to all parties.

Each party hereto will furnish one man to take steam cylinder indicator cards, which cards shall be taken at fifteen minutes' interval, and each of the parties hereto shall be furnished with every third indicator card taken during the test. The original record sheets will be copied on tracing cloth; the accuracy of the transcription shall be vouched for by each of the parties hereto and evidenced by their signatures on each of the tracings; that these tracings shall be made about 11x16 inches, with ¾-inch margin around the same, these tracings to remain in the possession of the party of the first part; and that the party of the first part shall furnish the parties of the second and third parts with six blue prints of each of the tracings made.

The party of the first part agrees to furnish all needed steam, water, oil, ammonia, chloride of calcium and other supplies to operate the plant during the intervals of the tests.

It is understood that during any and all of these tests and during the preparations made for these tests each of the parties shall be allowed to have representatives in and about the machinery to be tested; that these representatives may have the privilege of making notes of all that is being done in and about the plant to be tested, and furthermore that each or any of the representatives may have the privilege of protesting in writing to each of the other parties of these tests, these protests to be considered and taken into consideration when the final deductions or determinations are made as to the outcome of the tests.

It is also understood that at the expiration of the tests conducted by the parties of the second part the machinery shall be turned over to the party of the third part without any changes or alterations.

It is understood that the capacity shall be determined by the amount of ammonia handled, and also by the work done by the brine, that is, the number of gallon degrees of work done during the test. In order to avoid any possible mistake the specific heat and specific gravity of the brine shall be ascertained by Professor Hallock, of Columbia University, for each series of tests. The 80-ton brine cooler in the basement of building No. 12 of the party of the first part is to be used as the evaporating system.

De La Vergne Machine Company,
Adolph Bender, President.
Eastman Kodak Company,
J. H. Haste, Manager Kodak Park Works.
York Manufacturing Company,
Thomas Shipley, Vice-President.

Signed: January 4, 1908.
Approved: Louis Block.

In making up this agreement the Eastman Kodak Company was represented by F. W. Lovejoy, general manager Eastman Kodak Company; J. H. Haste, manager Kodak Park Works; P. S. Wilcox, assistant manager Kodak Park Works, and F. A. Cole, superintendent Kodak Park Works.

The De La Vergne Machine Company was represented by Louis Block, consulting engineer, formerly in the employ of the De La Vergne Machine Company; Prof. Charles E. Lucke, M. S., Ph. D., mechanical engineering department, Columbia University, and E. Dettmar, chief engineer De La Vergne Machine Company.

The York Manufacturing Company was represented by Thomas Shipley, vice-president and general manager; A. B. Strickler, manager sales department, and S. J. Shipley, New York representative.

Preparations for Test.

After the agreement was made the parties thereto immediately began to carry out their parts of the agreement in order to get the plant ready for the test.

The De La Vergne Machine Company reconstructed the compressors of its make of machine, putting in its latest design of discharge valves; putting valves on the by-pass close to the cylinder connections and reducing the clearance to a mimimum; taking out the 2-inch suction connections and putting in 2½-inch connections to each compressor; connecting up on the suction to each compressor a ½-inch liquid expansion valve, connecting same with a liquid ammonia main; and otherwise thoroughly overhauling the machine, so that the same would be in first-class condition.

The evaporating system which was used consisted of an 80-ton Hendricks type of cooler which had never been used before. This cooler, which was installed in one of the buildings of the Kodak Park Works, was connected to a triplex plunger pump driven by an electric motor.

It was necessary to install extra apparatus to heat the brine, so as to give the load for the machine. This apparatus consisted of a tank in which brine was heated and then circulated through three sections of a double pipe exchanger, through which was pumped the brine which was circulated through the evaporating system. With this apparatus it was possible to regulate the work being done by the cooler so as to maintain the back pressure going to the machine. The cooler was located about 400 feet away from the machine being tested, and was connected with it by a 4-inch suction main which was run outside the building and through the alleyways.

There was also an apparatus provided for weighing the ammonia going into the cooler. The liquid feeding this apparatus came from a general liquid main, and as there was no positive method of finding out the amount of ammonia in the cooler at the beginning or end of the test, the results of this apparatus cannot be taken as correct. The discharge from the compressors went into a section of the general condensers, the liquid condensed in same being passed into the general line feeding the entire compression system of the works.

Upon examination it was found that the De La Vergne machine was of the following dimensions: Compressors, double-acting, 11¼x22 inches; engine, Corliss, 22x22 inches.

On February 2, 1908, the De La Vergne compressor tests were started under the direction of the De La Vergne Machine Company's representatives. The first three days the machine was operated at forty-five revolutions per minute; the fourth day at thirty-four revolutions; the fifth day at fifty-six revolutions, and the sixth day at sixty-seven revolutions.

After the De La Vergne tests were completed the York Manufacturing Company took charge of the machine, removing the De La Vergne compressors and placing thereon single-acting compressors 15 inches in diameter by 22-inch stroke of the York make, connecting same up to the suction and discharge mains.

On March 6, 1908, the tests were started on the York compressors under the direction of the York Manufacturing Company's representatives. The first day of the series of tests made on the York compressors the temperature of the overflow water from the jackets was about 62° Fahr. The second day the water was about 75° Fahr., and the third day the jackets were filled up at the beginning of the test and no water was circulated through the jackets during the entire run. About 3 inches of water evaporated during the test. This was made up by putting in water with a bucket once or twice during the run to keep the water level about constant, the temperature of the water in the top of the jackets remaining at about 175° Fahr. For the fourth and fifth days' run the same conditions were maintained, so far as the jacket water was concerned, as on the third day, and on the sixth day enough water was circulated to keep the temperature of the overflow at about 75° Fahr.

It was impossible to make the sixth run in each test at a higher back pressure than given, because it was impossible, under the conditions, to get the required capacity out of the cooler.

The representatives of the different companies on the tests were as follows:

On behalf of the Eastman Kodak Company—F. A. Cole, W. S. Lucey, A. M. Lindsey, Jr., C. Gapin and J. T. Tallinger.

On behalf of the De La Vergne Machine Company—Louis Block, consulting engineer, formerly with the De La Vergne Machine Company; Prof. Charles E. Lucke, M. S., Ph. D., mechanical engineering department, Columbia University; E. D. Thurston, Jr.,

A. B., Mech. E., assistant mechanical engineering department, Columbia University; W. D. Monks, graduate engineering department, Columbia University; and E. Dettmar, chief engineer; W. Headley, superintendent erecting department; W. S. Simon, engineering and testing department; P. Laurenz, engineering and testing department; C. Krause, draftsman; Paul Diemb, erecting engineer, and C. W. Woods, erecting engineer, all of the De La Vergne Machine Company.

On behalf of the York Manufacturing Company—Louis S. Morse, chief draftsman; Abel Winborg, assistant mechanical engineer; Luther Bond, manager erecting department; H. R. Fisher, inspector; R. T. Whitehouse, draftsman; Ralph W. Bowers, draftsman; George Stark, apprentice draftsman; C. E. Allison, erecting engineer; George F. Robinson, erecting engineer; L. A. Ramsay, erecting engineer; F. X. Kuhn, erecting engineer; John Honey, erecting engineer; William Stair, erecting engineer, and William Troutman, machinist, all of the York Manufacturing Company.

Having placed the above facts on the records, I will proceed to discuss some of Professor Lucke's statements.

Under the heading of "The Cooling Load," the Professor states that the York Manufacturing Company had guaranteed the apparatus furnished to produce the load. This is not the fact. The company did furnish some parts of the apparatus needed to make the test, but certainly did not guarantee any part of it. Each party had men on the ground when the apparatus was installed, and each party had a right, in accordance with the agreement, to see that everything was put in to its satisfaction or make any alterations it saw fit, and to protest if not fairly treated, or to stop the test until it should receive fair treatment. The use of the apparatus, without protest or alteration, was an acceptance of it.

The De La Vergne representatives were very diligent in asserting their rights, and it is strange that they should neglect to protect their interest in regard to the method of heating the brine.

During the first five days of their test they had all the steam they needed and they made no complaint. It was only on the sixth day that they requested more steam, which was immediately given them, whereupon they demanded to be allowed to begin their run for that day over again. This was allowed them, and their first two and one-half hours' work for that day was thrown out.

The reason that the temperatures and pressures fluctuated more during the De La Vergne tests than they did during the York

tests was because the work was controlled by the "temperature" of the heating brine during the De La Vergne tests, while during the York tests the work was controlled by the "amount" of heating brine, that is, during the York tests the steam was allowed to flow into the steam coils heating the brine all the time, without any attempt being made to regulate the temperature of the brine, but the amount of this brine, which was allowed to enter the exchanger coils, was regulated so as to keep the work on the cooler as near uniform as possible.

The same method was used at York during both tests in which the De La Vergne Company took part in 1903, and there is no reason why that company could not have used this same method had it believed it could have obtained better results by doing so, it having charge of the entire plant during this test, and in addition it had run preliminary tests previous to starting the official tests, and had there been anything wrong with the apparatus it would have been discovered.

The charts show that the fluctuation complained of was just as bad, if not worse, on the fourth day of the De La Vergne test, when the work was only 36.8 tons, as it was on the fifth day of its test, when the work was 57.6 tons. This would go to show that the fluctuations were due more to the method of handling the plant than to the lack of steam, for if there was steam enough to produce 57.6 tons of work one day, there should have been no trouble on the day when only 36.8 tons of work was produced.

On the question of volumetric efficiency Professor Lucke has gone to a great deal of pains in his attempt to cast a doubt upon the accuracy of the data because the volumetric efficiencies of compressors do not follow their speeds in the same ratios regardless of design.

He talks about cylinder heating and travels back twenty years to a test made on still another type of machine, and upon this obsolete test he bases his claim that the volumetric efficiencies obtained by the York compressors were too high.

If Professor Lucke had looked up the records of the tests made at York in 1903, in which the De La Vergne Company took part, he would not have considered the efficiencies obtained at the Eastman tests very remarkable.

As to those obtained by the York compressors, the following might be said:

On the first day of the test the temperature of the water over-

flowing from the jackets was about 62° Fahr.; on the second day the temperature was about 75° Fahr.; on the third, fourth and fifth days the water stood in the jackets, but was not circulated, and on the sixth day the overflow water was about 75° Fahr.

On the first day the barrel of the compressor was heated up by the jacket water more than it was on the second and sixth days, and on the third, fourth and fifth days the water standing in the jackets helped to insulate them from the effects of the engine room temperature.

The effect of these conditions was a better efficiency on the third day than upon the second and first, due to the circulation of the jacket water during the first two days, which had a heating effect on the suction gas.

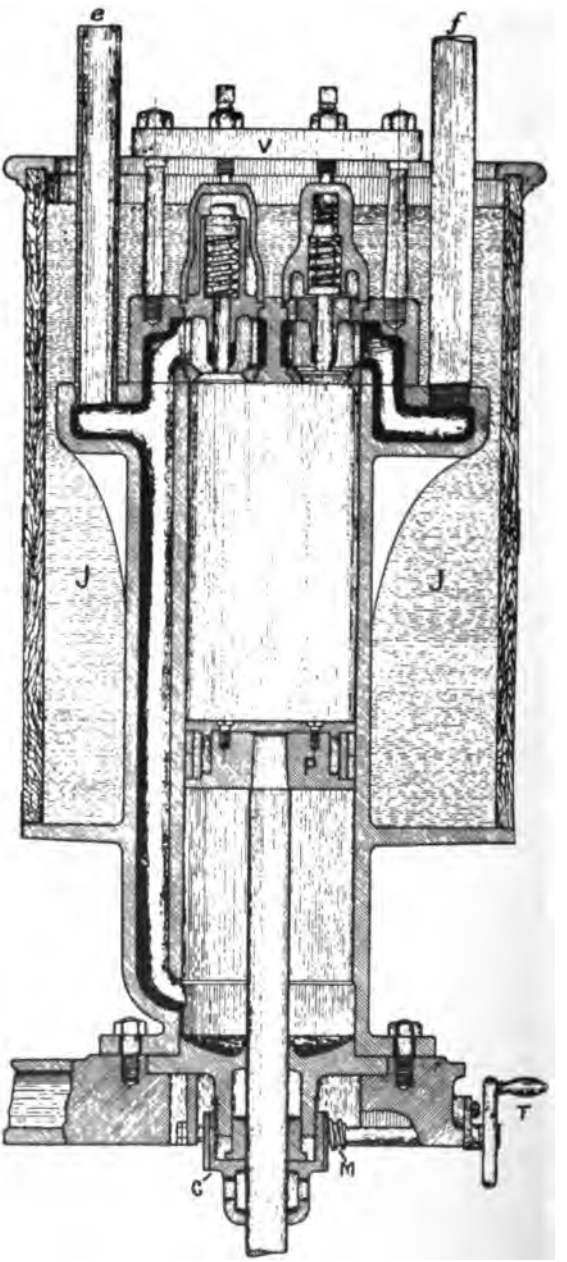
On the fourth day the slowing of the speed reduced the efficiency, while on the fifth day, the speed being increased, the efficiency was increased. On the sixth day the efficiency decreased, principally due to the circulation of the jacket water.

This may be new reasoning to Professor Lucke, but the members of this Society have heard of the effect of jacket water on efficiencies of compressors discussed, and they have also heard that the efficiency of a single-acting compressor, with a large, properly balanced suction valve, will increase with increased speed up to a very high rate of speed.

The efficiency of a compressor, no matter whether it is double-acting or single-acting, depends, first, upon the suction valve opening, and then upon the speed. The slower the speed, the greater will be the loss from cylinder heating, and the greater the heat head between the internal and external surface of the cylinder walls, the greater will be the loss from cylinder heating at all times.

The test Professor Lucke refers to as an authority was made by Professor Denton at the Knickerbocker Brewery, in New York, about 1890. The compressor used was a Consolidated or Boyle type of single-acting machine. A reproduction of this compressor is shown herewith. It was taken from Professor Denton's paper, published in Vol. XII of the Transactions of the American Society of Mechanical Engineers. The date of this volume is 1891.

This type of compressor is one of the least efficient of compressors. This is because the suction gas is churned through the channel connecting the suction port with the bottom of the compressor, into the bottom of the compressor, and then is forced back to the other end of the compressor into the valve chamber, which



CONSOLIDATED COMPRESSOR AT KNICKERBOCKER BREWERY TEST, 1890.

has only a thin dividing web to separate the suction from the discharge passages, and thence through a small suction valve into the compressor. In this way the gas is subjected to greater cylinder heating than in any other type of compressor. My experience with this type of compressor has proven to me that it is much less efficient than a double-acting machine where the suction and discharge valve chambers are not connected, as in the Linde type of compressor; hence the data obtained from such a machine twenty years ago are not the proper data upon which to base opinions, especially when it is known that the ammonia was metered and the back pressure was taken from a gauge.

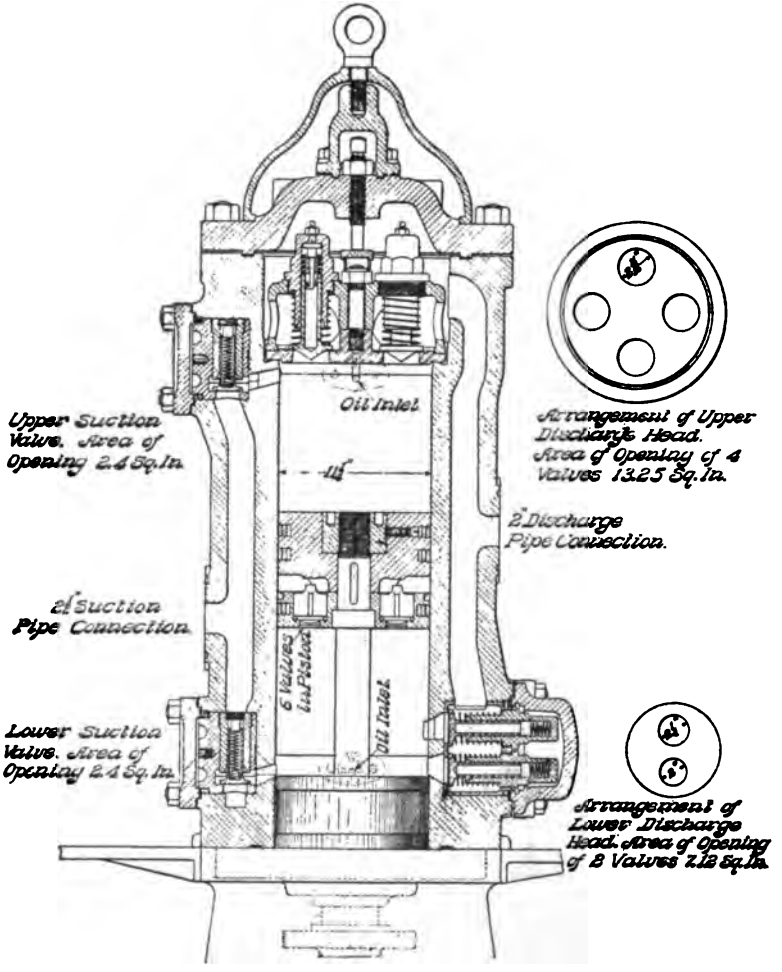
Another thing that Professor Lucke makes especial effort to condemn is the clearance of the York compressors. He shows by figures that the compressors must have had "one-seventh of an inch" clearance and he claims that this is so extremely small that it is dangerous to operate such a machine.

As to the clearance in the York compressor, I can assure him that it was not more than one-thirty-second of an inch, and hardly that, and, moreover, that one-thirty-second of an inch is what we call working clearance, and further I want to inform him that the De La Vergne compressors used in the Eastman test did not have any more than one-thirty-second of an inch clearance for either the top or the bottom. We know these things because when we took the compressors off we measured every detail carefully.

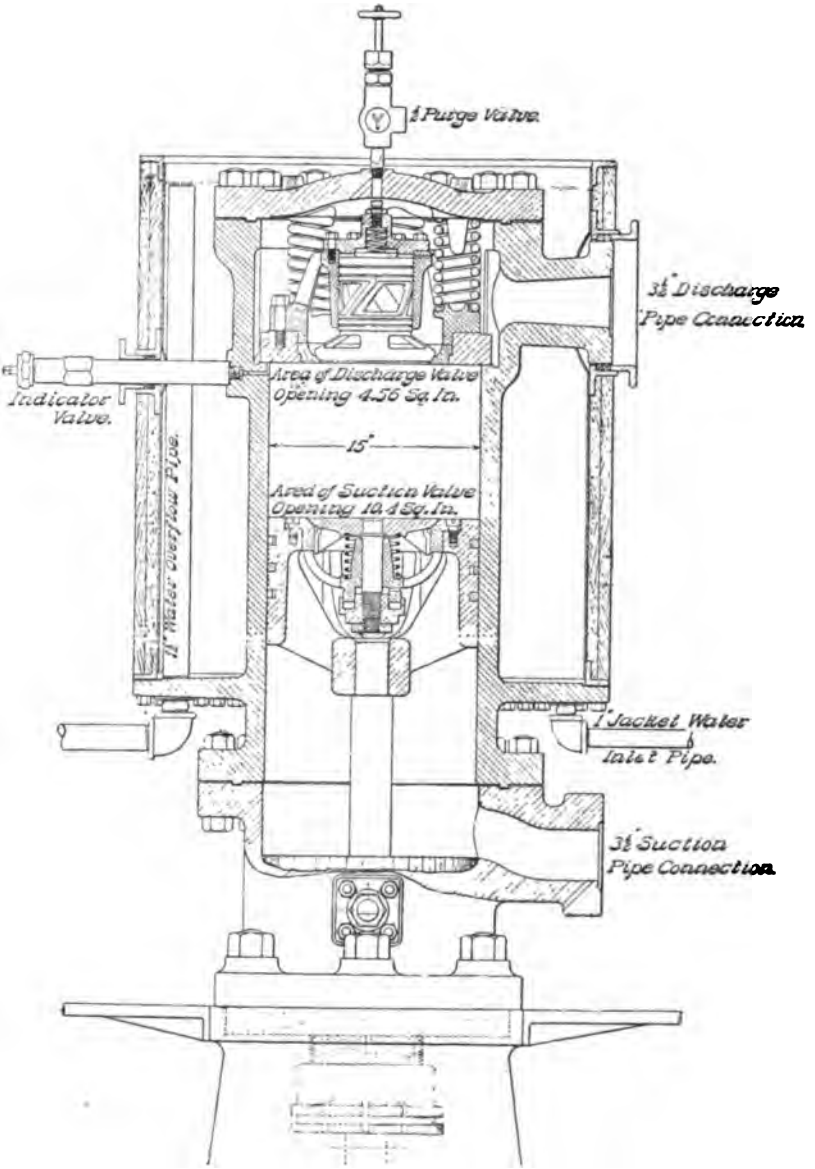
* The dimensions of the De La Vergne compressors are as follows: The distance from head to head is $30\frac{13}{32}$ inches; the piston measures from face to face 8½ inches; the stroke is 22 inches; so the only clearance was that made up by the gaskets in the bottom joints, the female of which was filled up with lead $\frac{5}{32}$ of an inch in order to give $\frac{1}{32}$ of an inch clearance at each end.

The new top valve head was faced off so as to allow it to project $\frac{5}{32}$ of an inch into the cylinder.

On the other hand, the York compressor measures 34¾ inches from head to head, and the piston measure from the top face to the bottom of the hub 12½ inches. Allowing 22-inch stroke and $\frac{1}{32}$ of an inch clearance on top and allowing $\frac{1}{16}$ inch for the gasket, leaves $\frac{9}{32}$ of an inch clearance on the bottom, which allows for lost motion and the wear of the bearings. With a spring safety head on top and $\frac{9}{32}$ of an inch clearance on the bottom, the question as to how the York compressor compares in safety with the De La Vergne compressor is easily answered.



De La Verne Compressor at Eastman Kodak Company Test, 1908.



YORK COMPRESSOR AT EASTMAN KODAK COMPANY TEST, 1908.

Another thing I want to call to Professor Lucke's attention is a paragraph on clearance in that "old familiar test" of Professor Denton's, to which he refers as an authority. The paragraph reads as follows:

"Clearance Spaces.—The piston traveled within 0.03 inch of the cylinder head during the period of about seven weeks covered by the experiments.

"By removing the inlet valve a sliver of sheet lead was laid upon the piston, and then the latter forced to the end of its stroke. The lead was then removed, and its thickness gauged. At the beginning of work, early in March, the thickness was 0.03. No adjustment was made during the succeeding seven weeks, the machine being run daily during that time.

"On April 30 a second clearance measurement was made, and the pistons were found to be 0.033 inch from the cylinder heads."

If Professor Lucke will look into the practical side of the question he has taken up, he will likely find a number of things which will show him that his chapters on "Cylinder Heating Effect" and "Volumetric Efficiencies" need quite a bit of revising.

As to the chapter on "Comparison of Indicator Cards," Professor Lucke has gone into a comparison of areas of the steam cards to quite a length, but I do not see that he has established anything but that the York compressors, as well as the De La Vergne compressors, were subjected to identically the same changes and variations. Both used the same engine, with the same governor and connections, the only change being in the compressors; hence the conditions would surely average up in ten-hour runs.

It might be possible to get a series of cards which would show equal power being used on both ends, but I have never seen many of such cards come from an engine driving an ammonia compressor. The work on the engine is not constant enough to keep the governor from racing a little.

One thing I am pleased to notice in Professor Lucke's comparisons, and that is, every York card shows less area for the same pressure than the corresponding De La Vergne card; in fact, Professor Lucke states that the De La Vergne cards always exceed the York cards in area for the same conditions by 4 to 6 per cent., and he attempts to justify this difference on the point of safety.

Now comes the "Equivalent Horse Power with Standard Conditions." However, Professor Lucke, after doing all this good work, throws cold water on the whole calculation just because it

shows a still further increase of performance of the York compressors over the De La Vergne compressors.

He seems to be astonished at the results which can be obtained by a single-acting compressor such as was used in the test we are discussing. On this point I would like to call his attention to a paper I read before this Society at the 1906 meeting. It is printed in Vol. II of our Transactions. In that paper I presented a table showing the horse power per ton for single-acting and double-acting compressors of the York Company make.

I stated that this table was one I had prepared for the use of our salesmen, and that it did not represent the best efficiency obtainable under the conditions specified, but that it was safe for general use. I want to say that the correctness of the table to which I refer has been further proven by the tests we have made since the time it was gotten up.

If you will look at that table under 20 pounds back pressure and 185 pounds high pressure, you will find that the efficiency for single-acting machines is given at 81.2 per cent. and of double-acting machines at 71.7 per cent. We obtained at that time and have obtained since 10 per cent. better efficiencies, but in getting up the table an allowance was made for safety.

If you add 10 per cent. to each of the efficiencies given above, you will find an efficiency of about what was obtained at the Eastman test—89.3 per cent. for the single-acting machine and 78.8 per cent. for the double-acting.

If you treat the horse power per ton the same and put on the friction obtained at the Eastman test, you will find that the test was not so extraordinary as it may appear to those who have not kept in touch with the data which have resulted from the tests we have made at York.

Now, it may be inferred that this 10 per cent. better efficiency is a new thought with me. On this point I will say that we have any quantity of data at York to prove this fact, and also that I made the same statement in public print, about the table referred to, some time ago.

Professor Lucke neglected to give the York compressors credit for the superheat which took place in the 400 feet of suction pipe connecting the compressors with the cooler. It may be that he did not want to discover any more items to the credit of the York compressors. It may be that somewhere in the calculations Professor Lucke has made he has given the York compressors credit for this work, but if he has I have failed to recognize it.

The following table will show what this amounts to. The figures are taken from Table XIII of Professor Lucke's paper:

TABLE I.

Heating of Gas in Suction Pipe.

		First Day	Second Day	Third Day	Fourth Day	Fifth Day	Sixth Day	Average
De La Vergne.	Average Temperature Suction Gas at Cooler	21.11	19.55	15.62	19.40	11.58	14.69	16.992
	Average Temperature Suction Gas at Machine	19.77	19.90	15.90	17.80	12.00	16.17	16.805
	Heat Imparted to Suction Gas Account Long Suction Pipe	−1.34	+0.35	−.43	−1.60	+0.42	+1.48	−0.187
	Average Temperature Outside Air	14.79	19.75	5.81	14.93	26.35	18.15	16.630
York.	Average Temperature Suction Gas at Cooler	9.333	9.36	10.37	9.29	9.90	13.20	10.242
	Average Temperature Suction Gas at Machine	15.155	14.28	16.79	14.77	13.52	18.155	15.445
	Heat Imparted to Suction Gas Account Long Suction Pipe	5.822	+4.92	+6.42	+5.48	+3.62	+4.955	+5.203
	Average Temperature Outside Air	37.21	37.40	41.05	24.79	27.82	49.48	36.292

You will notice that the average outside temperature during the De La Vergne tests was 16.6° Fahr., and that the average temperature of the suction gas at the cooler was about 17° Fahr., while the average temperature at the machine was 16.8° Fahr., which is a drop of 0.2° on an average between the cooler and the machine. Another thing you will notice is that the outside temperature was an average of 0.4° below the temperature of the suction gas at the cooler.

In the York tests you will notice that the average outside temperature was 26° Fahr. above the average temperature of the gas as it left the cooler, and that the average temperature of the gas at the machine was 5.2° warmer than when it left the cooler. This is an average difference of 5.4° between the two tests, due to the outside temperature. This means that the York compressors did considerably more work than they were given credit for by Professor Lucke in his calculations, nor has he made any attempt to show how the increased volume of the suction gas, due to this neglected superheat, affected the capacity of the York compressors; and this work which has been neglected is more than any loss that could have resulted by the fluctuations shown by the numerous charts Professor

Lucke has seen fit to bring out in his paper. It is, in my opinion, not out of order if we ask Professor Lucke why he neglected this point, and inquire if there are any other points he has neglected.

CONCLUSIONS.

First.—It is very hard to tell just what Professor Lucke means by his first conclusion, unless he means that the test, having proven that the York compressors use less horse power per ton than the De La Vergne compressors, not only proves the York compressors' superiority over the De La Vergne compressors but over all compressors which have small valve areas, no matter where they may be built, or, in other words, the test decided the question as to which type of compressor is the more economical to operate.

Second.—Professor Lucke in this conclusion questions the reliability of the data obtained at the Eastman tests.

I cannot agree with him in this deduction as to what are the proper temperature rises and what are high real volumetric efficiencies. The tonnage reported is not the full tonnage produced by the York compressors at the Eastman test, and even the full tonnage produced is not as large as could have been gotten out of the compressors had the conditions been different.

Third.—His claim that the horse power is not correct within 10 per cent. is a mere supposition, as are his other probabilities or opinions as to what the relative horse power would be for the De La Vergne and York compressors. I do not think it is possible for Professor Lucke to produce a record of any other De La Vergne compressor showing anything near as low horse power per ton as was shown by the De La Vergne compressors on this test.

Fourth.—His statement as to the value of water jackets is correct. This may be a new fact to Professor Lucke, but it is not to the members of this Society who have followed up the papers presented at our meetings.

Fifth.—Professor Lucke's conclusions as to what is a safe clearance for a compressor, no matter what its design may be, show very clearly that he has not even taken the trouble to inquire as to the clearance at which the De La Vergne compressors were operated during the test under discussion.

Sixth.—All the conditions of the tests were favorable to the small valves of the De La Vergne compressor. The speeds were low, the back pressure was high, and their compressors and pistons were worn as smooth as glass; while the York compressors were new, right out of the shop, with none of the surfaces worn down. The

apparatus was used during the De La Vergne test without an intimation of unfair treatment, and every request or protest was granted. When the tests were concluded the company's representatives seemed overjoyed with the results.

Shortly after the De La Vergne tests were concluded Professor Lucke came out in a public print and called attention to the fact that the "tests on vertical double-acting compressors were carefully checked," and further stated "that the test was very carefully executed," and he wound up by saying: "These tests are so extremely interesting and were made in such great detail, etc." Everything was lovely because the company thought it was impossible to beat its record.

No one said a word about "lack of steam" or "unfairness" until noon of the last day of the York test, when it was too late to make changes to prove that the claim was unfounded, as other unwarranted claims of theirs had been disposed of. When the other two parties to the test offered to make an extra test, removing all the objections complained of, the De La Vergne representatives refused to be a party to the proof test, showing by their actions that they believed their claims were unfounded. The extra test was made, however, on March 14 with only the Eastman and York representatives present. The conditions for this run were the same as for the first three tests of each series, and all objections complained of by the De La Vergne Company were removed. The results of this test were equal to the best previous run under the same conditions.

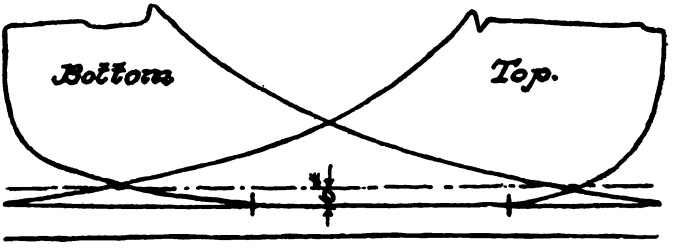
A report of this test was included in the report of the Eastman Company, but I find, for some reason, Professor Lucke has left it out of his paper, and he makes no mention of it whatever.

Seventh.—Professor Lucke's remark as to the possibility of discharge gas from an internally cooled compressor being colder than that from a water jacket compressor is correct.

Eighth.—The precise value of liquid injection may not be known, but I do know that without it the De La Vergne compressors would not have shown the efficiency they did.

Ninth.—This conclusion is the same as Professor Lucke's first conclusion, and I again agree that the difference between the De La Vergne and York compressors lies in the fact that the De La Vergne compressor has suction valves which are not large enough to allow the compressor to fill properly with ammonia, even at very slow speeds, whereas the York compressor has properly designed suction valves which allow the ammonia to fill the compressor at all speeds.

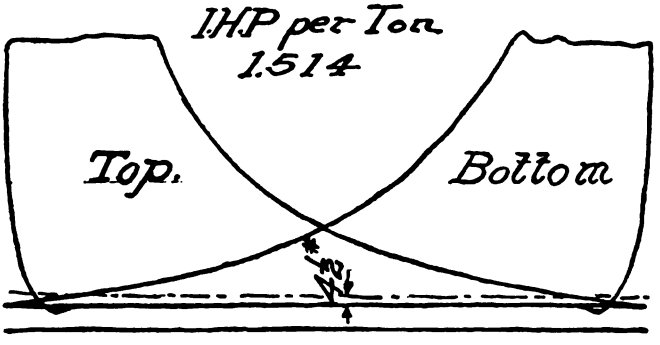
Now, in answer to Professor Lucke's general summary. He claims that the De La Vergne compressor was very nearly as good at the end of seventeen years' service as a new York compressor. Again



Compressor card, De La Vergne machine: 28 lbs. gauge suction pr.; 126 lbs. gauge condenser pr.; 49 r.p.m.; scale 1″ = 100 lbs. Nov. 27, 1907.

Professor Lucke is in error. An indicator card taken from the De La Vergne compressor on November 27, 1907, when this controversy first came up, is shown herewith.

This card shows that while the suction pressure was 28 pounds, the pressure inside the compressor was 18 pounds. The revolutions were 49. Look at the re-expansion shown. What do you imagine the efficiency of the compressor was at this time, and what was the horse power per ton, and this was not after seventeen years' work, for the machine had been overhauled by the De La Vergne Company one year previously, so it was the wear and tear of one year?



Compressor card, De La Vergne machine: 20.2 lbs. suction pr. by mercury column; 186 lbs. gauge condenser pr.; 45 r.p.m.; scale 1″ = 120 lbs. Feb. 4, 1908.

Look at the indicator card, dated February 4, which is a card given to our men by the De La Vergne representatives during the De La Vergne tests, as part of our record. The machine was run-

ning at 45 revolutions. Do you suppose it did not take a lot of overhauling to get the compressor in shape to produce a card like that? That is where the new valves and the general overhauling came in. It had not been running for seventeen years in that condition. No, nor seventeen days. Those compressors, after they were fitted up, were in better shape than a new set. The walls of the cylinder were as smooth as glass and the piston and rings were par excellence. As to the valves, they were ground and reground until they must have been in the best condition possible, and even during the tests those valves were taken out and fitted up several times, so the records shown by the test are not those of seventeen-year-old compressors, but of compressors newly fitted up and kept up, or they would not show anything near the efficiency they did, which the De La Vergne representatives admitted had never been equaled before; and it must not be forgotten that the oil connections were made larger before the high speed test was made, and that the clearance was reduced to a minimum; that liquid injection was introduced and the conditions under which it was operated could not be duplicated in practice. It was, in fact, more of a laboratory condition than a working condition, while the York compressors were run under working conditions and were run from end to end of test without being opened or adjusted, showing that the York compressor could meet any condition without alteration or adjustment, while the De La Vergne could not.

To the query as "to the life of a York compressor," I will say that over seventeen years ago I replaced a pair of 14x28 inch double-acting compressors on a De La Vergne machine with a pair of 17x28 inch single-acting York compressors, and those compressors are doing as good work today as they did seventeen years ago, setting there on the De La Vergne columns and driven by a De La Vergne engine, and they are doing work that the 14x28 inch De La Vergne compressors could not do, and they are still in the prime of life.

From the above facts it can be seen that the knowledge that there is a vast difference between these types of compressors is no new idea. I have known of the difference between the suction valves for many years, and the result of that knowledge is shown by the fact that I have been instrumental, directly and indirectly, in changing nearly a hundred machines of the De La Vergne make, taking off their cylinders and replacing them with single-acting.

An idea of what can be gained by such a change can be had by any one of you who will go to the Waldorf-Astoria Hotel. There is a plant which had three De La Vergne machines, but now each

one of these machines has single-acting compressors. They have been running with the York compressors for the last four years, and two of them have been doing more work than the three formerly did when they had double-acting compressors. Now they have a spare machine, where before all three machines could not do the work.

Professor Lucke says even if there is a difference in the horse power per ton, it is better to pay this as a "price of safety." Cuts of both compressors are printed herewith, and I would like the Professor to point out wherein the safety lies in the construction of the De La Vergne compressor, with all its small valves, and the lack of clearance. Where is the safety to justify the loss of horse power?

Compare it with the cut of the York compressor. Anyone who is at all familiar with the trade knows that the York type of compressor is commonly known as the "safety head compressor." That is one of the points of that type of machine which recommend it to the purchaser.

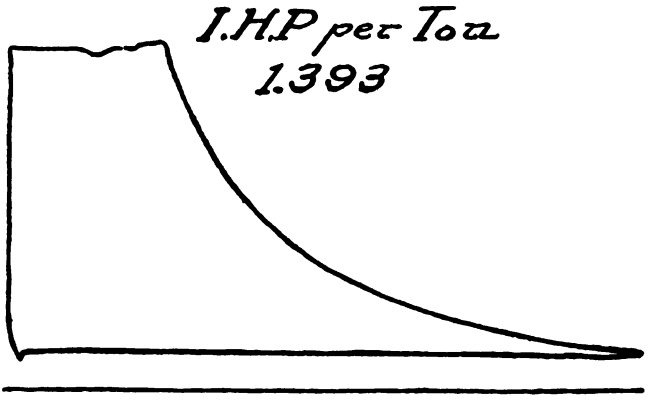
If safety is desired, then the York compressor gives both safety and less horse power per ton.

Now comes the question as to the price to be paid for the alleged safety.

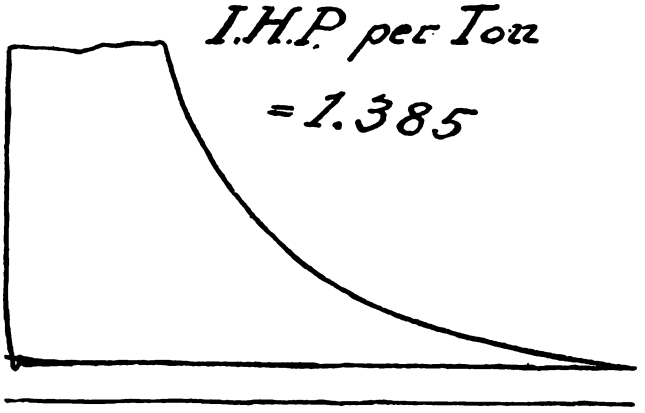
Here are a series of compressor cards from the Eastman test:

The left column consists of York cards.

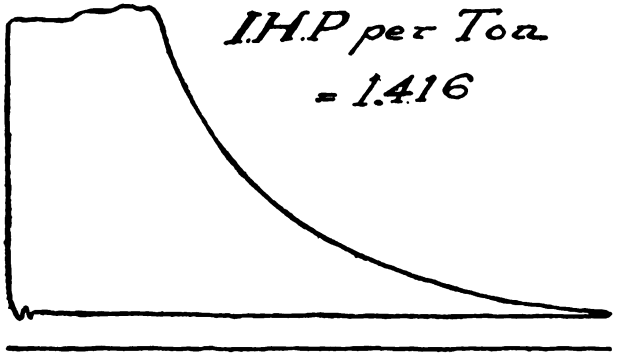
The right column is made up of De La Vergne cards.



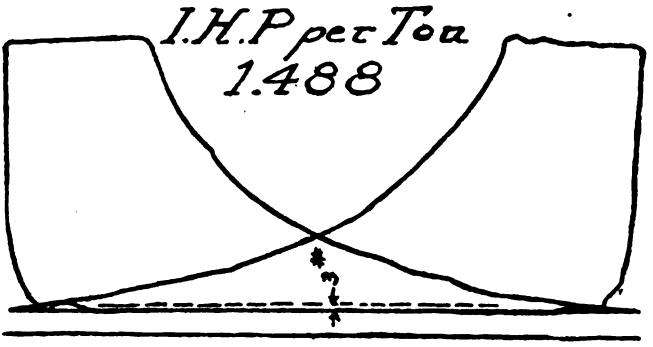
Compressor card, York machine: 19.625 lbs. suction pr. by mercury column; 187 lbs. gauge condenser pr.; 34 r.p.m.; scale 1″ = 100 lbs. March 9, 1908.



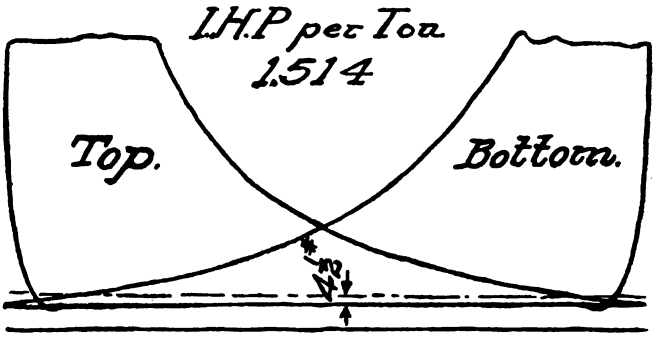
Compressor card, York machine: 20.15 lbs. suction pr. by mercury column; 188 lbs. gauge condenser pr.; 45 r.p.m.; scale 1″ = 100 lbs. March 8, 1908.



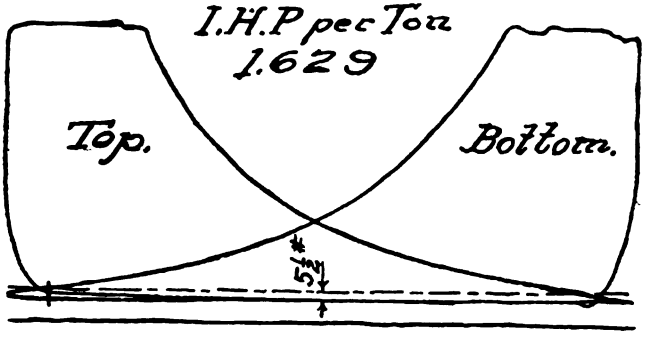
Compressor card, York machine: 19.44 lbs. suction pr. by mercury column; 188 lbs. gauge condenser pr.; 56 r.p.m.; scale 1″ = 100 lbs. March 10, 1908.



Compressor card, De La Vergne machine: 19.75 lbs. suction pr. by mercury column; 186 lbs. gauge condenser pr.; 84 r.p.m.; scale 1″ = 120 lbs. Feb. 5, 1908.



Compressor card, De La Vergne machine: 20.2 lbs. suction pr. by mercury column; 186 lbs. gauge condenser pr.; 45 r.p.m.; scale 1″ = 120 lbs. Feb. 4, 1908.



Compressor card, De La Vergne machine: 20.5 lbs. suction pr. by mercury column; 185 lbs. gauge condenser pr.; 56 r.p.m.; scale 1″ = 120 lbs. Feb. 6, 1908.

Professor Lucke's Table XXI gives the horse power per ton for the De La Vergne compressors at 34 revolutions as 6.8 per cent. more than the York compressors at the same revolutions.

At 45 revolutions the De La Vergne compressors required 9.5 per cent. more power than the York compressors at the same revolutions.

At 56 revolutions the De La Vergne compressors required 15.1 per cent. more power than the York compressors at the same revolutions.

The power per ton refrigerating duty required for the De La Vergne compressors keeps increasing as the speed increases, while the power required to drive the York compressors remains practically the same per ton for all speeds.

I have stated that the test was favorable to the De La Vergne compressors, and the above records show what I mean.

Had the speeds been those speeds at which the York compressors are most frequently operated, viz., 60, 70, 80 and 90 revolutions, the difference in power required would have been much more marked. The principal cause of this difference can readily be seen in the cards before us, which show the difference between the pressure in the suction pipe and the pressure in the compressor during the suction period, for the De La Vergne compressor, to be 3 pounds at 34 revolutions; 4½ pounds at 45 revolutions and 5½ pounds at 56 revolutions, while the York compressors' cards show practically no difference at any speed.

This is all accounted for in the design of the suction valves.

This difference in the power required is a very heavy price to pay for safety you do not get, but this is not the total price you pay; for you are called upon to purchase and install about twice as much machinery, if you want to operate the same within even reasonable economy, as would be necessary with machinery having proper suction valve area.

On the question of engineering, can it be held for a moment that it is good engineering or modern engineering to design a piece of machinery which must be run at one-half the capacity which the same machine could produce if a change was made in the design of the compressors?

If this better design was not well known there would be some excuse for its not being accepted, but when it is known and has been used for years, and it has been proven again and again, not only by test but practical demonstrations, and when owner after owner has had the capacity and the economy of his plant increased by adopting

the improvements in the design of his compressor, where does the good engineering come in?

If other proof is needed to show that Professor Lucke is wrong in his deductions let us take up the subject of clearance again.

He makes a direct statement that because his figures lead him to believe that the York compressor has one-seventh of an inch clearance he condemns that compressor as being unsafe, and advises us that the clearance is too small for reliable commercial operation.

Does he not know that the printed instructions given by the De La Vergne Company to its outside foremen, dated December 1, 1902, direct those men that the standard clearance for their vertical oil compressors is one-sixteenth of an inch on the top and one-eighth of an inch on the bottom, and for dry compressors the size of the York compressors used at the Eastman tests, should be one-sixty-fourth of an inch on the top and three-sixty-fourths of an inch on the bottom.

This clearance is about one-tenth of the clearance he condemns. Does he condemn not only the practice of the York Manufacturing Company and the De La Vergne Machine Company, but the practice used in the trade generally? Possibly he will explain his position.

There is one thing I want to call attention to and that is this—while some of us may not agree with Professor Lucke in his deductions, let us not forget that his has been a hard task.

Professor Lucke's attempt will benefit good engineering, as it will lead many men, who have rested peacefully under false conclusions, to wake up and get into the procession and thereby get a greater benefit from their investments.

It is my belief that the paper which Professor Lucke has presented, instead of being a "Scotch verdict," is an "epitaph" on all compressors having small suction valves.

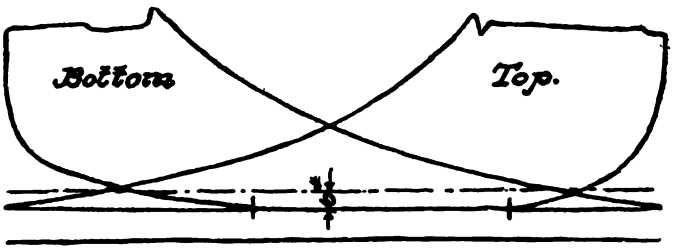
*Charles E. Lucke.**—The keen interest shown in the discussion of this paper is very gratifying to the author, but the tendency of the discussion to dwell on issues not raised in the paper is a little disappointing. The most elaborate discussion is by Thomas Shipley, and as the speaker has dilated at such length on certain points his discussion requires detailed notice. Before, however, examining it in detail it is desirable to review the real issue of the paper.

In view of the apparent elaborate nature of the Eastman tests, and the possibility of the placing of a misconstruction on the results, the paper concerns itself with, first, an examination of the data on

* Author's closure under the rules.

which the results are based for the purpose of judging the reliability of the data; second, an analysis of the results with a view to showing how reliable are the results on the horse power per ton refrigeration; third, assuming that both data and results are correct and accurate, how far does a difference in horse power per ton bear upon the relative values of the compressors.

A superficial examination of the data reveals no discrepancies, so it is not surprising to find a tendency to rebel against intricate analysis of these data on account of it being intricate or against a conclusion that the data are incorrect, especially when the proofs of unreliability require some little study to understand. The body of the paper is concerned with the derivation of criteria for judging not only the accuracy, but the significance of each set of data, and with the application of each criterion in the proper place, from all of which it appears that not only are the relative figures on horse



Compressor card, De La Vergne machine: 28 lbs. gauge suction pr.; 126 lbs. gauge condenser pr.; 49 r.p.m.; scale 1″ = 100 lbs. Nov. 27, 1907.

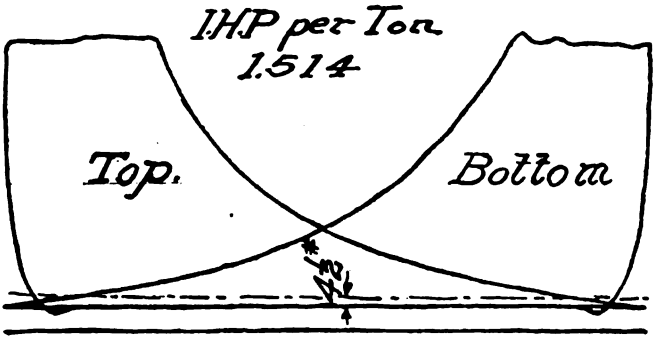
FIGURE 1.

power per ton not to be relied upon, but that the two machines did not operate under identical conditions. As the significance of operating conditions, foreign to the construction of the compressor, had been previously established, the fact that unequal conditions maintained for the two machines make it necessarily follow that even if the data were reliable, which they were not, the results in horse power per ton were not comparable, and any conclusions on the relative values of the compressors, based primarily on the relative values of the horse power per ton for the two machines, are entirely unjustified and worthless.

To this line of reasoning the speaker has not addressed himself directly, but has, on the contrary, construed the paper as being an attack on the York compressor, and upon the fairness of the conduct of the test on the part of the Eastman representatives, and on such assumptions the discussion has wandered far afield. There was ab-

solutely no intention on the part of the author to attack a machine which has so many admirable qualities, but on the other hand it was shown that the results do not condemn the other equally fine machine, as the speaker so sweepingly states. Nor was it the intention even to hint at unfairness on the part of the Eastman representatives, but, on the contrary, it was shown that the inequality of conditions resulted in spite of all the most careful efforts of men fully qualified and unquestionably fair, by reason of uncontrollable conditions, foreign entirely to the compressor.

In view of the objects of the author and the line of argument of the paper reviewed above, the contribution of the speaker will be found to be unsound. The significance and meaning of some of



Compressor card, De La Vergne machine: 20.2 lbs. suction pr. by mercury column; 186 lbs. gauge condenser pr.; 45 r.p.m.; scale 1″ = 120 lbs. Feb. 4, 1908.

FIGURE 2.

the most important facts presented by the author are misconceived by the speaker.

An examination of parts of the discussion will result in the elimination of nearly all the arguments offered by the speaker in opposition to the position taken by the author, and will show that the main thesis of the paper has not been in the slightest degree weakened.

Evidence of error in facts by the speaker is to be found in his citation of the indicator card taken from the De La Vergne machine previous to the test. This card, Fig. 1, shows an enormous re-expansion and an excessively large pressure drop through the suction, leading the speaker to say dramatically, "Look at the re-expansion shown"! This card is cited by him as proof that the De La Vergne compressor was in very bad condition just previous to the test, and that it was very considerably overhauled for the test,

and that it had not operated seventeen years in good condition. These conclusions are all based on the interpretation to be placed on the re-expansion and the excessive pressure drop, and if the speaker's interpretation is wrong so also must be his conclusion. Now the excessive pressure drop is due to an incompletely opened suction cock, and could not have been due to anything else because the suction valves were in nowise changed between the taking of this horrible example card, and the other one, Fig. 2, with which the speaker has chosen to compare it. It was perfectly easy for anybody to partly close one of these cocks, and to get an even more horrible example of suction resistance than this, and that a man of the speaker's experience should not know this is almost unbelievable. With regard to the re-expansion part of it, more significance is indirectly attached, as one is led to believe that such enormous re-expansion proves very great clearances, which were later reduced for the test. There is ample proof on record in the Transactions of this Society that the speaker is in error. That this excessive re-expansion is not due to re-expansion of gas in the clearance, but to evaporation of liquid ammonia, and that the speaker knows it, is shown by the following quotation from his own paper before the Society in 1906, referring to a card which is here reproduced in Fig. 3:

"The excessive re-expansion is due to the presence of liquid in the clearance space."

From this it appears that the speaker is capable of at one time explaining the peculiarity of this line as due to liquid ammonia when it refers to his own machine, but finds it impossible to explain the line when it refers to the other machine.

The following case will suffice to show how completely some of the most important arguments of the paper escaped the speaker, and led to his presentation of much material in opposition to what he thought had been said, all of which seems quite convincing to a superficial reader, but it has nothing to do with the paper. For example, the speaker, after offering a table showing how much the gas was heated in the suction pipe between cooler and compressor, made the statement:

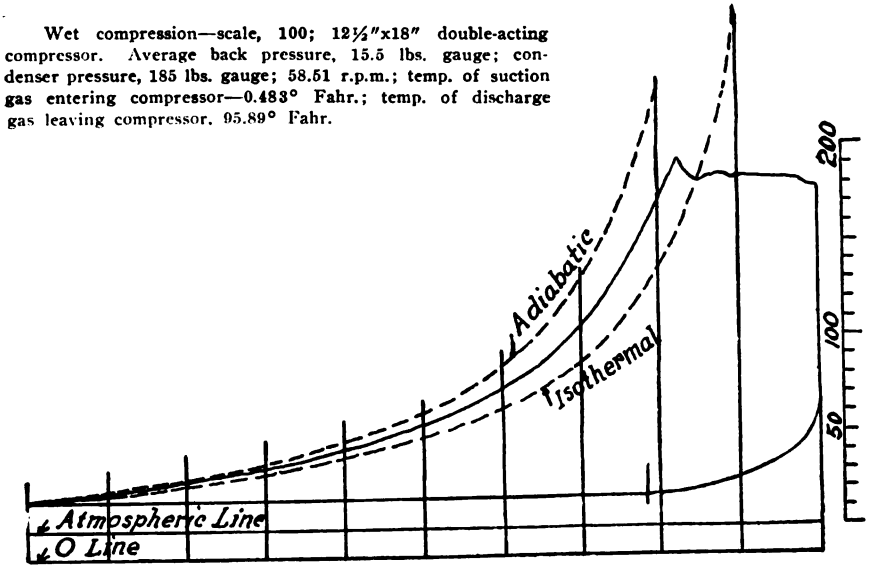
"It may be that somewhere in the calculations Professor Lucke has made he has given the York compressors credit for the work, but if he has I have failed to recognize it."

Having thus cautiously approached the subject, the speaker as-

sumes that it has not been taken into account, and adds the following:

"This means that the York compressors did considerably more work than they were given credit for by Professor Lucke in his calculations, nor has he made any attempt to show how the increased volume of the suction gas, due to this neglected superheat, affected the capacity of the York compressors," etc.

In other words, the speaker first says he is not quite sure whether the author did or did not take it into account, and later quite positively and somewhat severely takes the author to task for neglecting it. In Table XIV the tonnage actually produced for the



Crank end—21.6 per cent. re-expansion loss.

FIGURE 3.—(REPRODUCED FROM 1906 "TRANSACTIONS," PAGE 166.)

day, March 8, in which the superheat on the suction line complained of was at maximum, was 53.2. This is figured from the brine itself. The ammonia that would produce this 53.2 tons under the conditions maintaining in the cooler and at the expansion valve would yield 10,900.4 cubic feet of vapor per hour under the conditions maintaining at the compressor, as shown in Table XV. In other words the compressor took from the suction line, at whatever conditions of superheat that may have existed in the pipe, 10,900.4 cubic feet of vapor per hour. This compressor, capable of taking this volume of vapor, whether superheated or saturated, was found to be able under the standard conditions of 34.7 per square inch absolute to take 1,371.1

pounds of saturated vapor. When this weight of ammonia is evaporated in the cooler without line losses there will be produced 51.84 tons refrigeration. This is the equivalent tonnage at standard conditions, and includes all corrections, debits and credits, including that for the vapor superheat, neglect of which was charged.

Another equally strong case of misconception is concerned with the fluctuation of conditions of brine inlet temperature and temperature range at the cooler, suction pressure and temperature at the cooler, and the inequality of these fluctuations in the two series of tests, constituting in effect a handicap against the De La Vergne machine. That these fluctuations existed is proved conclusively by the curves in Figs. 1 to 4 in the paper, and that they are unequal and favor the York machine is seen at a glance. The existence of this inequality of conditions is not denied by the speaker, so that by implication it is admitted that the results of the two series of tests, being, therefore, based on unequal suction conditions, are not comparable. This is one of the main contentions of the paper, and supports one of its strongest conclusions, and the fact that it is not denied shows that either it was admitted or its significance had not appealed to the speaker. On the contrary, there is much space in the discussion devoted to the secondary question of why the fluctuations did exist. As a matter of fact it does not make a bit of difference why they existed; the fact that they were there makes the results not comparable, and judgment based upon the results unfair, misleading and unwarranted. With regard to the reasons for the fluctuations, however, the speaker is likewise at fault. Assuming that this reason assigned by the author was shortage of steam in the secondary brine heater, the speaker says:

"The charts, Figs. 1 to 4, show that the fluctuation complained of was just as bad, if not worse, on the fourth day of the De La Vergne test, when the work was only 36.8 tons, as it was on the fifth day of their test, when the work was 57.6 tons."

And concludes that it could not have been shortage, and so must have been due to mismanagement. The reason for the fluctuations was not so simple a condition as shortage of steam, and the paper clearly states this fact. All the arguments, therefore, based on the assumption that the irregularity was charged by the author to shortage when it was not are pointless and would never have been introduced, nor would his other alternative of incompetent operation, if the speaker really understood the statement of the paper quoted below:

"It was found that during the De La Vergne series the steam surface was insufficient and the steam supply to the heater fluctuated most unexpectedly, so

that the operator, finding his brine getting cooler, would open the steam valve, hoping to raise the temperature, whereas a lack of supply of steam permitted it to continue to drop, and then, without warning, steam would come, and before the valve could be adjusted again the brine had become too warm. As a consequence, therefore, during these runs the brine inlet to the cooler had a temperature impossible to control at will, and through no fault of the operators or builders of the machine."

It thus appears that while insufficiency of surface was part of the trouble, and which part was eliminated by increasing the surface before the York series, the really serious element was not the shortage of steam or the incompetence of the operators, but the unexpectedness with which the available supply varied. To assume, as the speaker does, that such sudden jumps in available steam supply could have been met by the magic method of "quantity" control used by the York men is quite absurd, because it would involve a variation of pump speed to match the available steam supply; the quantity of heat taken up by the brine from the steam could not be controlled any better or as good by varying pump speed than by varying steam valve openings.

That the facts presented by the paper may not be comprehended by the speaker may be likewise demonstrated by that part of the discussion directed toward the explanation of the erratic figures on real volumetric efficiency. The true volumetric efficiency is less than 100 per cent. by reason of loss in pressure in drawing gas into the cylinder, and by the cylinder heating effect during and previous to suction in the cylinder, so that changes of volumetric efficiency, being dependent on the physical laws of heating gases and loss of pressure through orifices, must necessarily follow some consistent law. In the York compressor it is quite certain that both speed and jacket temperature will affect the result, but the change must be consistent, so that with constant jacket conditions of standing water, if an increase of speed from 34 to 45 R. P. M. causes a rise in volumetric efficiency, as it does, then there should be a similar increase in volumetric efficiency in further increasing the speed from 45 to 56 R. P. M., whereas it actually decreased. As stated in the paper, such inconsistency is a prime reason for doubting the accuracy of the figures, and it is an absolutely unassailable reason. That the force of this argument has completely escaped the speaker is shown by the following quotation, applying to three of the days when water was standing in the jackets, and hence constant jacket conditions existed:

"On the fourth day the slowing of the speed, from 45 R. P. M. to 34 R. P. M., reduced the efficiency from .894 to .854 = .04, while on the fifth day,

the speed being increased from 34 R. P. M. to 56 R. P. M., the efficiency was increased from .854 to .871 = .017."

Why, then, with reasoning so satisfying to the speaker as to lead him to state that it is probably new to the author, does it happen that in comparing the third and fifth days, where the speed increase was from 45 R. P. M. to 56 R. P. M., the efficiency falls from .894 to .871 = .023, and so contradicts this reasoning? As a matter of fact, reasoning that contradicts itself is new to the author, and the argument of the paper that inconsistency in these figures is convincing proof of their unreliability is not only uncontradicted, but really quite effectively supported.

To the arguments of the paper on cylinder heating, supporting the two contentions, first, that the figures are erratic and unreliable, and that, therefore, the data on volumetric efficiency from which they are derived are also unreliable, and, second, that the heating effect is too small, and the volumetric efficiency and tonnage which follow are too high, the speaker refers by statements that will, when reviewed, show more misconception and contradiction. He says:

"The slower the speed the greater will be the loss from cylinder heating."

provided jacket conditions are constant; or inversely, increase of speed causes a decrease of suction heating. This the author believes to be true, and the fact that the figures of Table XVIII are in opposition certainly condemns the figures. Comparing the second and sixth days, when jacket water was circulating, and at approximately the same temperature, it will be found that increase of speed from 45 to 67, or about 50 per cent., caused not a decrease, but an increase of cylinder heating; similarly, comparing the third and fifth days, when the water was standing in the jackets, a speed increase from 45 to 56 caused even a greater increase in heating effect. Thus, not only is the author's position on the erratic and unreliable nature of these data uncontradicted, but the very argument advanced in opposition is really the strongest kind of support.

Regarding the belief of the author that the heating effect is too small because it ranges between 45° and 72°, when suction gas is between 13° and 19°, and discharge gas is between 216° and 253°, and the interior walls somewhat hotter, even with ample opportunity for heat exchange, both in the lower part of the cylinder and in transferring to the highly heated top, the speaker makes no direct reference. This is circumstantial evidence of the strongest sort that these heating effects are too small, and the volumetric efficiencies and tonnages too high, but it is not even noticed. An indirect reference is made by the speaker in his assumption that the old Consoli-

dated figures are introduced in support of the author's position, whereas they were incorporated only because of their interest as being the only figures in existence with all other conditions producing them fully specified. That they are of little value in judging modern machines is certainly true, and the author would never have been guilty of advancing them to support an argument quite capable of standing alone.

If still another case of misconception be necessary to prove how general it was on the part of the speaker, it can be found in his interpretation of the significance of the steam indicator cards. In the body of the paper indicator card figures were inserted with a view to showing how reliable they may be considered. Two bases of comparison, absolutely unassailable, were used and both yielded the same results. The importance of this is shown also in the paper when it is pointed out that out of 615 steam admissions only one record card is taken at the standard speed of 45 R. P. M., and the percentage of representation is much smaller at the higher speeds because fifteen-minute intervals were used, and the speeds ranged from 45 to 67 R. P. M. It was shown that by reason of an unadjusted valve setting a certain relation between head and crank end cards should maintain, and that it did maintain for considerable portions of time is proved by frequently recurring relations between the card areas on the head and crank ends. That it did not maintain at all times is shown by considerable departures from the recurring relations, or to put it in plain language, while at times the difference between 6 and 4 was 2, at other times it was 1 or 3. This is all the proof that is necessary to establish the fact that some of the indicator cards used in the computations were not fair samples of fifteen-minute intervals, or that one card taken as the representative of over 600 possible ones for the past fifteen minutes did not represent it. To this convincing argument the speaker addresses the statement:

"It might be possible to get a series of cards which would show equal power used on both ends, but I have never seen many of such cards come from an engine driving an ammonia compressor."

This may be. It is an interesting observation that the speaker introduces, but it has nothing to do with the case. The point is that at some time every day the head and crank end cards were equal, while at other times they were very far from being equal under what were supposed to be absolutely identical conditions. To say, as the speaker did, that the work on the engine was not constant enough to produce consistent results between head and crank end

card is to assume that proper test conditions had not been reached. This was not the case during the test; or, if it were the case, it is all the more reason for throwing out the data as useless. If at the moment of taking a card something happened to the operation of the compressor to accelerate the governor movement, positively or negatively, either by interfering with the governor belt by hand, or by manipulating steam valves or ammonia valves, then most certainly that particular card must be thrown out, because it cannot even by a remote possibility be claimed to represent the average of a fifteen-minute interval. This fact also shows how completely irrelevant is another of the speaker's statements that "conditions would surely average up in ten-hour runs." The only things that fix the load of the steam cylinder, and, therefore, the speed and the governor position, are the suction and condenser pressures, and these certainly were not subject to any sudden fluctuations so as to cause the governor to move. If they were, and if a card were taken at that time, then the areas reported for the cards are not true averages for the periods, and the contention of the author that the data are misleading is, therefore, supported. In other words, the arguments advanced by the speaker to support his contention really support the arguments of the paper. The speaker further asserts in reference to these indicator cards that, admitting the fluctuations, both machines were affected alike. How anyone examining Table XIX, which compares these cards, and understanding it, can possibly assert that both machines were affected alike by the fluctuations is past understanding.

Finally, the really significant thing about these cards is that they had variable relations when by all the rules of judgment they should have been constant, and this is not denied, and furthermore, that the amount of departure from constancy when constancy should exist is a measure of unreliability, so that the 10 per cent. thus determined as the extent of the error in some of the indicator cards and horse power is not a "mere supposition," as the speaker charges.

Perhaps more attention was devoted by the speaker to arguments based on a misquotation concerning clearance, with a view to showing how entirely wrong was something that the author did not say, than to any other single thing in his contribution. Quoting from the speaker's discussion, the following is what he assumes was said in the paper, referring to the author:

"He shows by figures that the compressor must have had one-seventh of an inch clearance, and he claims that this is so extremely small that it is dangerous to operate such a machine."

As a matter of fact the author never said this, so that practically all of the arguments presented to attack the assumed statement were aimed at a target that did not exist. What the author did say can be found by referring to the original paper, under "Cylinder Heating Effect," which is here reproduced:

"The apparent volumetric efficiency taken from the York ammonia cards averaged about 98 per cent., and no difference could be definitely fixed for different cards because of the thickness of the lines and the smallness of the distances; this value has, therefore, been taken as most probably correct for all runs. This is equivalent to only 2 per cent. re-expansion, which, by computation, appears to be equivalent to a clearance of less than 1 per cent., or about .7 per cent."

Clearance is here used, as cannot possibly be misunderstood by anyone skilled in this particular art, to mean volume in cubic inches or volume in per cent. of the displacement. Continuing the quotation of the author's statement about clearance:

"If the cylinder and piston were both truly round and of uniform diameter at the top, and the piston and cylinder heads truly flat, then the linear clearance corresponding to this volume would be about one-seventh of an inch, an extremely small value."

This is very different from saying that the clearance is one-seventh of an inch.

It is particularly significant to note here that the original paper went on to explain how the linear clearance would become less than the equivalent one-seventh of an inch corresponding to this same .7 per cent. volumetric clearance, as the piston and cylinder head become more and more irregular, departing from the truly flat, but no limit was set, as this obviously will depend on the extent of the irregularities of surface.

It was also assumed by the speaker without warrant that the author had associated the possibility of danger with small linear clearance, while as a matter of fact the danger conditions are well set down in the paper:

"Safety in handling cylinder liquid demanded clearance in which it could do no harm, and safety from broken suction valves demanded that they be located in pockets."

There is no implication here that linear clearance has of itself anything to do with safety, but in comparing the means of the two compressors, where one uses clearance and the other a safety head to avoid liquid danger, and one a side pocket to hold a suction valve and the other a piston-seated valve, it was stated incidentally and purely as a matter of opinion that the clearance method was safer

than the small volumetric clearance spring-head method, as the former did not involve the operation of any mechanism. The latter became a safety element only when a heavy head, having very considerable inertia, had been moved by the liquid shock and caused it to lift against the full discharge pressure. In the spring-head type the liquid shock is first produced, and later relieved, while with sufficient volumetric clearance the shock is avoided, as it is quite obvious that if the clearance volume is equal to the volume of the liquid there can be no shock at all. That the speaker held an opposite view was to be expected.

That the arguments tending to show that the horse-power data are unreliable, and that the suction heating figures are not only unreliable but too small, volumetric efficiency too high, and tonnage data, therefore, erratic and too high, all of which combined make the figures for horse power per ton, are in error and based on unequal conditions, and hence not comparable even if correct, have been unassailed in detail is shown by the preceding analysis of the speaker's contribution. Not only are the elements entering into the result denied by the speaker and opposed by reasoning that fails on analysis to support the speaker's position, but similarly weak arguments are addressed to the result itself. As examples of weak reasoning the following is a good example: The speaker says that the data and result criticised must be correct;—

a. Because of the value of the stake;

b. Because they are 10 per cent. higher than figures previously claimed in the paper he read before you in 1906;

c. Because the representatives on the test were so eminently competent.

The last is especially amusing in view of the fact that the De La Vergne representatives were in another place declared incompetent as a reason for the fluctuations of brine cooler conditions, so that the same men are declared to be alternately good and bad by him to suit the particular issue of the speaker.

With regard to the details of the conduct of the test, protests against the unfairness and errors of management, the author has nothing to do nor has the paper. The author's position is simply that of a judge who, on analyzing the evidence concerning the data, announces a decision that the results of the tests are inconclusive because the data are unreliable, and who is not concerned with how the evidence was obtained or with the squabbles between actors or witnesses, except so far as they result in recorded real evidence.

The more general question of the relative value of the compressors was not brought before the Society by the author, as this is largely a matter of opinion, not of the respective builders, but of users and owners, and cannot be settled by any such analysis as can be applied to horse power per ton. The question was, it is true, touched upon, but merely to show that horse power per ton is one element in the judgment of this larger question, and only one of many, and that the relative value of compressors is no more to be settled on the horse power per ton basis of comparison than is the relative value of engines on the thermal efficiency or fuel consumption per horse-power-hour basis. For if this were the criterion, all steam engines would be scrapped in favor of gas engines, and even if confined to steam engines alone 90 per cent. of all steam engines would be scrapped because their fuel consumption so far exceeds that of the other 10 per cent; the absurdity of both conclusions supports the argument that this efficiency criterion is not the deciding one in the case of engines, and it is no more true that the horse-power-per-ton criterion can decide the relative value of compressors.

No. 58

DOES AMMONIA DISINTEGRATE IN ABSORPTION PLANTS?

By Herman Dannenbaum; Philadelphia, Pa.

(Non-Member of the Society.)

This question, which your program committee desires to have discussed, was asked at last year's meeting and was answered in the negative. The same answer has to be given again, as any possible temperature in an absorption plant is far below the temperature at which ammonia will decompose.

Ramsey and Young, in their experiments in 1884, found the temperature of decomposition as about 900° Fahr., while White and Melville, in later experiments, results of which are published in the Journal of the American Chemical Society for 1905, determined that ammonia will be decomposed at a temperature close to 850° Fahr. They made their experiments under various conditions, and while it would be a difficult, if not impossible, matter to duplicate in a laboratory experiment all the conditions existing in an absorption plant, the results of their experiments warrant the conclusion that no serious decomposition of ammonia takes place in an absorption plant.

Abnormal consumption of ammonia in absorption plants is undoubtedly due to the same cause as in compression plants, namely, to leaks. But absorption plants appear to have one specific difficulty to contend with, and that is the formation of so-called "foul gas," which is a non-condensable gas collecting in the condensers and occupying there valuable space. To get an explanation of the formation of this gas is probably the reason why the question about decomposition of ammonia was asked. In my opinion this so-called foul gas is hydrogen gas, and is formed by decomposition of water, probably through electric currents. We find a very good indirect proof for this opinion in the fact that operators of absorption plants find in their stills smaller or larger quantities of oxide of iron. Ammonia contains no oxygen, and we have to look to water as the

source from which the oxygen originates, the water being decomposed into its constituents, oxygen and hydrogen.

Every winter we receive from operators of absorption plants samples of black sediments, which they remove from their stills, and invariably we find it to be black oxide of iron. I wish to mention here that quite frequently we get the samples in the shape of a black greasy material, and generally the statement comes with it that "it must be tar, which could only come from ammonia"; but we find it to be black oxide of iron mixed with lubricating oil, the oil undoubtedly entering the still through the piston rod of the aqua ammonia pump.

Hydrogen is an inflammable gas, and so is generally the foul gas formed in absorption plants. The removal of this foul gas from a system may be the cause of a serious loss of ammonia. Pure ammonia gas blown from a small valve opening cannot be ignited, but if a lamp or candle is held to such an opening the heat is sufficient to decompose part of the ammonia into hydrogen and nitrogen, the amount of decomposition depending upon the size and heat of the flame. The hydrogen gas may burn with a peculiar wavering flame. As soon as the light is removed this will stop. I have mentioned that foul gas from an absorption system can be ignited, and many operators of absorption machines believe that if the gas which escapes from a purge valve on the condensers, burns, they remove only foul gas from the system. This is a mistake. I have made a number of experiments with various mixtures of hydrogen and ammonia gas, and found that a mixture of 15 per cent. hydrogen gas and 85 per cent. of ammonia gas by volume can be ignited and will readily burn. The flame appears to be hot enough to decompose the entire amount of ammonia gas in the mixture, as no ammonia smell can be noticed. This means that if an operator of an absorption plant removes one cubic foot of foul gas from his condensers in this manner he may at the same time remove from them 5 cubic feet of ammonia gas.

The only proper place from which to remove foul gas from an absorption plant is at the absorbers. It would be necessary to have a pipe line—a one-quarter inch pipe would probably be sufficient—from the condensers, where the foul gas accumulates, to the absorbers. Whenever the presence of foul gas is indicated by abnormal high condenser pressure, the condensers could be purged through the absorbers, where at least most of the ammonia gas would be absorbed, while the foul gas would pass away.

It would be desirable to purge only one condenser coil at a

time, and the top and bottom valves of the coil should be closed and the coil flushed with cold water before the valve in the purging pipe is opened.

DISCUSSION.

Albert Johnson.—Referring to Mr. Dannenbaum's theory of the electric current decomposing the water and forming the foul gases, do not these foul gases occur in plants where there are no electric currents?

H. Dannenbaum.—I doubt it.

Albert Johnson.—Is that assumption or fact?

H. Dannenbaum.—It is an assumption. I do not know what else will cause the decomposition of the water.

Albert Johnson.—I do not know that any effort has been made to find this out.

H. Dannenbaum.—I do not believe anybody would intentionally apply the electric current to any still of an absorption machine.

Albert Johnson.—I did not know but what it had been tried lately.

H. Dannenbaum.—Not that I know of.

A. C. Bishop.—Mr. Dannenbaum, is the burning of foul gas at the absorber an assurance of loss of ammonia?

H. Dannenbaum.—No, certainly not, if you have an absorber under a back pressure, as it is generally. If the absorber is under a pressure of 5 to 10 pounds—naturally ammonia gas will be mixed with the foul gas and there will be some loss of ammonia gas, only not the same amount as if you would burn it off from the condensers. If you want to prevent the loss entirely you would have to have a separate absorber, and you would have to blow the gas from time to time through this special absorber and absorb the ammonia gas and blow the gas off. But then you would get probably a lot of weak aqua ammonia, which would be of very little value to you, because aqua ammonia below a strength of 16° or 18° Baumé has practically no value for the absorption machine. It could only be sent to the manufacturer for reworking, and it would not always pay to do that.

Albert Johnson.—Aren't there any other gases besides what you term hydrogen gas? Aren't there many hydro-carbon gases formed in the absorption plants?

H. Dannenbaum.—When I wrote the committee that I would

try to answer their question it was my intention to go to a number of absorption machines and take samples of the so-called foul gas and analyze them to see if the suggestion I make about the formation of the foul gas would be proven by actual analysis. But I did not find time, and I asked the committee if they would not wait for another year. The committee requested me to read the paper as it was, and to try to submit additional information next year. Of course only an actual analysis of the gas would prove that my theory is correct.

Henry Torrance, Jr.—I would like to ask Mr. Dannenbaum about the disintegration in compression plants. Is there any there?

H. Dannenbaum.—No, I don't believe there is any perceptible disintegration in a compression plant. The claim is only made when a man has no ammonia left in his system, and he does not know what became of it. He does not know or does not want to admit that he has leaks, and he says the ammonia decomposes. If ammonia decomposes the volume would increase, and there should be considerable gas in the system. Now you find air in a compression plant, but not, I believe, this gas, which can be ignited and burns by itself, as it does in an absorption plant. At least I never heard that claim made.

Gardner T. Voorhees.—It would seem from Mr. Dannenbaum's remarks about the disassociation of ammonia in the absorption plant —which I think are absolutely correct; that is, the water disassociates into hydrogen and oxygen, and with the iron forms oxide of iron—if that is so in the absorption plant, then you never would have to buy any more ammonia for the absorption machine, except what was lost through leaks, because if there was any ammonia formed it would be free ammonia, and you would only have to add water to make up the deficit.

H. Dannenbaum.—That is not only true of the absorption machine; it is true of the compression machine.

Albert Johnson.—Referring to the foul gases again, I am quite satisfied that these foul gases are caused by other gases which occur in the original raw material and hydro-carbon gas, which would be developed to just as great an extent as the hydrogen would be developed by this process which Mr. Dannenbaum explains. In fact, our company has gone to great expense from time to time to produce a gas which does not contain or contains as very little of this hydro-carbon gas as possible in the aqua. As to the expense of this aqua, it

makes it cost more, and therefore it has not become a commercial article; the users won't pay for it. But if they would pay for it, they could get an aqua ammonia which had very nearly all these hydrocarbons eliminated before they get to the liquid.

H. Dannenbaum.—The question could be settled by using an absolutely chemically pure aqua ammonia in some small plant.

No. 59.

REFRIGERATION APPLIED TO AIR SUPPLY FOR BLAST FURNACES.

By Bruce Walter, Pittsburg, Pa.

(Member of the Society.)

Of all the industries of modern times there is none of more importance than the production of metallic iron from its various ores, and none plays a more important part in the development of modern arts and sciences. Within the present generation the advances in the production of pig iron, both as to method and quantity, have been remarkable. A few years ago a furnace which produced 200 tons of pig iron per day was a wonder, but at present one which produces less than 450 tons per day is considered small.

The cost of making a ton of pig iron has been reduced in inverse ratio with the increase of production by the installation of ore-handling machinery, automatic top filling, pig-casting machines and other labor-saving devices. The regularity of output has been taken care of by the careful analysis of the ores, coke and limestone and the introduction of scale cars, which are frequently tested to insure no mistakes in filling the exact quantity of materials required to make the quality of product desired. The one element that has been neglected until recently is the air used for the purpose of furnishing oxygen for the combustion of the coke. There are required, approximately, 2 tons of ore, half a ton of limestone and a ton of coke to make a ton of iron. The air required to be blown per ton of iron is about 5 tons, or 43 per cent. more than the combined weight of the ore, coke and limestone. This air is ordinarily taken from the blowing engine room without regard to moisture content or temperature, and is measured by the piston displacement of the blowing engines. As it is desired to have a fixed quantity of oxygen for the combustion of the coke, and the weight of the air blown per revolution varies indirectly with its absolute temperature, it is readily seen that this method of measuring is rather crude when compared with the careful treatment accorded to the ore, coke and limestone. In addition to this, and vastly more important, is the moisture content of the air taken into the blowing engines. All air in its natural state con-

tains water vapor in varying quantities, dependent upon the temperature and the opportunity which the air has for acquiring moisture.

In the Pittsburg district the moisture ranges from a minimum of less than half a grain per cubic foot on an extremely cold winter day to a maximum of 9.5 grains per cubic foot in midsummer. The variations from day to day cover a very wide range, not infrequently changing three or four grains during a period of twenty-four hours. One case is on record of a drop of moisture from 8.51 grains to 6.14 grains in less than two hours. This irregularity in the moisture content of the air not only materially changes the quantity of oxygen entering the furnace by the displacement of air with water vapor, but each pound of water requires about 13,000 B. T. U. for its decomposition into oxygen and hydrogen. This heat, together with the heat required to raise the temperature of the vapor to the point of decomposition, must be furnished by the coke, and is not only a waste of energy but a continual disturber of conditions within the furnace, as with high moisture the heat available for smelting decreases and with low moisture increases. This condition was noted by many furnace operators years ago, but no serious attempt was made to provide a remedy until 1897, when the Carnegie Steel Company began some experiments to determine the approximate cost of removing the moisture by means of refrigeration. These experiments were conducted under the general supervision of James Gayley, who was then a managing director of the Carnegie Steel Company, and who is the holder of the patents covering the Gayley dry blast process. These experiments were conducted more or less continuously for three years; first by use of a 4-ton and later of a 20-ton refrigerating machine, in connection with insulated boxes containing pipe coils over which the air was circulated. An approximate determination was made of the quantity of refrigeration and square feet absorbing surface necessary to accomplish the work of maintaining a uniformly dry air supply under the worst atmospheric conditions.

The construction of the first dry blast plant for practical work was begun in 1903 at the Isabella plant of the United States Steel Corporation, and was designed to handle 40,000 cubic feet of air per minute. This plant was put in operation on August 11, 1904, and was operated with very few interruptions until November 29, 1907, when the entire furnace plant was shut down on account of the decreased demand for steel. The plant is equipped with two duplex, single-acting ammonia compressors driven by direct-connected, cross-compound steam engines. The rated capacity of these machines is 225 tons each. The ammonia condensers consist of thirty-seven coils of

the atmospheric type, and the brine coolers are of the double pipe type, submerged in a brine tank. The refrigerating apparatus has absolutely no special features and is arranged and operated in the same manner as are hundreds of other plants for the cooling of brine. The refrigerating chamber for cooling and drying the air consists of a brick and steel building, 44 feet long, 28 feet wide and 36 feet high. The walls of this building are lined with cork board, which is finished on the inside with a heavy coat of pitch. The interior is divided into four compartments by cross walls, and each compartment contains a built-up coil of 2-inch pipes, 20 feet in length. These coils are 30 feet 6 inches high, and the pipes are staggered, so as to insure the frequent impingement of the air upon the pipe surface. The whole quantity of pipe in the refrigerating chamber is 100,000 lineal feet. The brine is fed into the coils at the top, and after making a run downward through 1,800 feet of pipe, it is returned to the brine coolers. The air which is to be cooled and dried is introduced into a basement under the refrigerating coils by a steel plate blower at a pressure of one and two-tenth ounces per square inch. From the basement this air passes upward over the refrigerating coil, where, due to the reduction of temperature, a portion of the water vapor which it originally contained is deposited on the pipes in the form of water or frost. The dry air then passes through valves in the roof of the refrigerating chamber and is taken to the blowing engines through a pipe 6 feet in diameter. If the air is introduced into the chamber at 90° Fahr. temperature and 8½ grains moisture per cubic foot it will leave the chamber at 32° and contain 2 grains of moisture. The temperature of the brine entering the chamber will be 16° and 33° when leaving it.

The moisture determinations are made by use of the wet and dry bulb thermometers inserted in two pipes, one of which is connected to the discharge of the blower and the other connected to the dry air pipe leading to the blowing engines. The slight pressure on the system causes a very decided current in these pipes when open to the atmosphere, and in this manner we get the same effect on the wet bulb thermometer as is produced by whirling it in the open air, which is the practice at all United States Weather Bureau stations. The actual amount of moisture is then determined by reference to the United States Weather Bureau tables. The accumulation of frost and ice on the coils must be removed every fourth day, and as our chamber is divided into four compartments it is our practice to thaw off one compartment a day. To do this we close the air valve at the top of the compartment in order to stop the air circu-

lation; next, the brine inlet and outlet valves are closed and the brine is drained from the coil into a tank in the basement; water from the ammonia condenser is then sprayed over the coils by means of a permanent arrangement of sprinkler nozzles near the roof of the building. After the frost is removed the brine is returned to the coil by means of a pump, and after the pipes are filled the cold brine is put in circulation, and a few minutes later the air valve is opened, putting the compartment in operation again. The whole operation requires from two to three hours. The quantity of water removed from the air used by a blast furnace making 450 tons of iron per day amounts to from 5,000 to 6,000 gallons in twenty-four hours when the humidity is near its maximum.

The advantages gained by this treatment of the air are manifold. The saving of coke has been shown to be about 350 pounds per ton of iron. The daily output is increased about 10 per cent., with no increased labor cost. The temperature of the waste gases coming off the top of the furnace is lowered, showing that more of the heat of the coke is absorbed by the stock. The regularity of the quantity of oxygen introduced at the tuyeres keeps the temperature of the furnace more nearly constant, and this condition makes the iron produced more regular, which is of great value in the manufacture of steel, and even more important in the manufacture of foundry iron for the open market. The reduction of the temperature of the air decreases its volume, and therefore the number of revolutions of the blowing engines is decreased, requiring less steam for their operation, and this saving in steam is more than is used in the operation of the dry blast plant. In the operation of furnaces with earthy ores there is considerable loss, due to the fact that the fine particles of ore are carried out of the top of the furnace with the waste gases. This loss is much greater on a furnace that is working in an irregular manner, and the application of dry air has reduced this loss by 35 to 40 per cent. There is also a decrease in the quantity of limestone required to produce a ton of iron, as by the reduction of coke the quantity of ash is reduced and less lime is required to take care of it. It is also stated by experienced furnace men that the furnace lining will have a longer life in a furnace using dry than in one using natural air, due to the greater regularity of operations. There has not been time to demonstrate this fully, but the claim does not seem unreasonable. When all these advantages are summed up there is found to be quite an appreciable decrease of cost in the making of pig iron by the application of dry air. On the other side of the account we have the cost of operation of the plant itself, exclusive of

the steam, which, as has been stated above, is more than saved in the blowing engines. This cost is insignificant when compared with the saving effected, not amounting to as much as 10 per cent. There is also a slight reduction in the calorfic value of the waste gases due to a more complete combustion of the coke. The exact amount of this deterioration has not been fully determined, but it is probable that the increased regularity in the quantity of gas will more than compensate for the slight loss of calorific value.

Since the installation of the Isabella plant there have been installed four other plants which are now in successful operation. The first of these installations was built at the furnaces of the Warwick Iron and Steel Company at Pottstown, Pa. This plant was put in operation on August 7, 1907, and is designed to take care of 70,000 cubic feet of air per minute, which is used in two blast furnaces. The second installation was put in operation at the E. & G. Brooke Iron Company's No. 3 furnace at Birdsboro, Pa. This plant was started on August 19, 1907, and has a capacity of 20,000 cubic feet of air per minute. The third dry blast plant was built at the Dowlais Cardiff Works, Cardiff, South Wales. This plant is designed for 70,000 cubic feet of air per minute for use in two furnaces, and was put in operation on November 2, 1907. The fourth plant was installed at the South Works of the Illinois Steel Company, and is located at South Chicago, Ill. This plant was put in operation on May 3, 1908, and was first applied to three furnaces to test its capacity. It was found that the plant would successfully remove the moisture from 122,000 cubic feet of air per minute. After operating several weeks one of the furnaces was put on natural air and the surplus dry air was used to blow the Bessemer converters. These require almost as much air as a blast furnace, so the load on the dry blast plant was only slightly reduced. The results of the application to the Bessemer process were very gratifying, improving the quality of steel produced and permitting the use of lower silicon iron. In fact, everywhere that the process has been tried it has shown results which compare favorably with those obtained at the Isabella furnaces.

In addition to the plants already installed there is now in course of construction a plant for the Northwestern Iron Company, of Milwaukee, having a capacity of 46,600 cubic feet, at its furnace plant at Mayville, Wis.

The Toledo Furnace Company, of Cleveland, Ohio, is also building a plant at its furnace plant at Toledo, Ohio. This plant is to have a capacity of 81,000 cubic feet of air per minute. A third plant is being built by the Cleveland Cliffs Iron Company at Mar-

quette, Mich, and is being designed for 10,000 cubic feet of air per minute.

A number of other companies have secured licenses to use the process, and at present there are under consideration no less than seven large plants having a total capacity of over 1,000,000 cubic feet of air per minute.

It seems certain that the iron and steel makers have taken up the process as a necessary appliance to their work, and there is no doubt that in a few years a furnace without a dry blast plant will be considered a back number. It is also possible that the process will be found of great value in cupola work, and we may yet see the day when at least the larger foundries will be in the market for refrigerating machinery.

DISCUSSION.

Louis Block.—Mr. Walter, you made the statement that the horse power saved in the blowing engines was more than was necessary to drive the cold blast plant. How many revolutions less does the blowing engine run now than it did before the installation of the dry blast plant at the Isabella furnace, and do you attribute all this saving of air to the fact that the air is now taken into the blowing engine at a lower temperature, or do you attribute it also to the fact that less oxygen is required for combustion in the furnace—whether one or the other or both?

Bruce Walter.—The reduction in revolutions of the blowing engine at the Isabella furnace was from 114 to 96 per minute. Part of that, slightly over half of that, was due to reduction in temperature of the air, and the consequent greater density. The other part was due to the burning of less coke requiring less oxygen.

Louis Block.—That would mean about 13 per cent. less revolutions, would it not?

Bruce Walter.—Yes.

Louis Block.—The entire horse power required to compress 40,000 cubic feet of air per minute, which you are using, I understand, would be about 2,500, if you reduce that 13 per cent.

Bruce Walter.—Make that 2,700.

Louis Block.—Make that 2,700 and take 13 per cent. of that, there would be a reduction of 325 horse power?

Bruce Walter.—Yes.

Louis Block.—That amount of horse power would not begin to run two 225-ton refrigerating machines under the conditions under

which you operate, beside operating the blower and the brine pumps. Isn't that so?

Bruce Walter.—That statement of mine needs some qualification. We have found by very careful figures and from very careful indication of the refrigerating machines and all the auxiliaries, the blowing engines, and also calculating the tonnage of refrigeration from the compressors, and assuming the horse power per ton, that in a year we save more than the amount of horse power required to operate the refrigerating plant with the auxiliaries. You must remember that we have to run our full capacity only a few months in the summer. There are months in the winter that we barely turn over the machine, and days on which we do not run at all. In other words, our load factor, we might say, is about 50 per cent. of the maximum load. For that reason 325 horse power which we save in a year is ample for the operation of the dry blast plant. My statement refers only to steam consumption, not to boiler capacity necessary to operate the plant at its maximum condition.

Louis Block.—Steam consumption per year?

Bruce Walter.—Yes.

Louis Block.—That explains it.

D. S. Jacobus.—I would like to ask Mr. Walter a question. He says that the daily output is increased about 10 per cent., and possibly he can tell us how this is effected. A short time ago a party writing a work on metallurgy came to me and I found that he had nothing in the book about refrigeration. I asked him why he left it out. "Oh," he said, "the German authorities have gone over all that, and have figured but very little saving, and I do not think it worth while to put it in." Does the advantage claimed come from heat saving, or through being able to operate the furnace with a greater degree of regularity with the dry air? I notice that the name of the furnace in which the apparatus is installed is the Isabella. I have had little to do around iron plants, and I once asked a man well up in the art why so many furnaces were named after women. "Well," he said, "a furance is very cranky. It will be running along smoothly, and, when everything seems in the best of shape, it will suddenly go wrong, and the only way you can get it back is to humor it." Anybody that has been around a furnace and seen a few severe "slips" will appreciate this.

Bruce Walter.—Dr. Jacobus is right about the regularity. That is the great part of it. A furnace that is operating well requires extra coke every now and then, and that coke is charged up at the end of the month to operation, and increases the total quantity of coke

required to make the iron, and while the furnace is not working properly it is making either poor iron or a very small quantity of it. I do not know just what Dr. Jacobus refers to in the 10 per cent. saving.

D. S. Jacobus.—Well, I have been told that when figured from a thermal standpoint the gain is somewhere around 3½ per cent., whereas the actual gain is in the neighborhood of 10 per cent. or over on account of other features. Is this so?

Bruce Walter.—This has been figured by different metallurgists, men well up in their profession, at anywhere from 3½ to 9 per cent., theoretically. Nine per cent. is, I believe, the highest that was figured. We claim a saving of coke of about 15 per cent., for the average, for a long time period. The saving has been as much as 20 per cent. for short periods, but for long periods 15 per cent. is about the maximum. We increase the output at the same time, as the paper says, about 10 per cent., which is quite an addition to the profits of operating a blast furnace.

Gardner T. Voorhees.—Mr. President, I would like to get a chance to say something about multiple effect compressors in this connection. I want to say, in applying it to the blast furance, that some figures that we have been looking over lately on the actual results obtained from the multiple effect compressor show that if a 1,000-ton refrigeration plant were built for a blast furnace, that instead of the cooling of the air and the abstracting of the moisture with one back pressure, as is done now, as I understand it—that is, the refrigerating machine is operated on a back pressure sufficiently low so as to cool the air to at least 32°—that air can be cooled in two or more stages; that is, in two-stage multiple effect compressors, it can be cooled from 90° to 50° and from 50° to 32°, and can do that easily, working at two back pressures. If that were a 1,000-ton refrigerating plant I believe I will not be far off in saying that the first cost in compressors, if made multiple effect, would be reduced by $50,000, and with the coal to operate the compressors at $3 a ton, the saving in a year by use of the multiple effect compressor would be about $40,000. That is what would be saved in a plant with a multiple effect compressor.

Thomas Shipley.—I believe Mr. Voorhees is in error when he assumes we do not use stage compression for blast furnace work. We not only use two-stage but we use four-stage compression.

Louis Block.—I have a clipping out of a paper here which will answer the question by Dr. Jacobus. Edward B. Cook, of Warwick Furnace, of Pottstown, Pa., says: "It was admitted for years that

the dry blast would save only 3 per cent. of coke. But the fact was neglected that this 3 per cent. of the total fuel would be saved to the hearth alone, and that this would amount to four times as much; that is, 12 per cent. of the heat in the hearth, and as the hearth is the only weak place, the efficiency of the furnace would be increased 12 per cent."

No. 60.

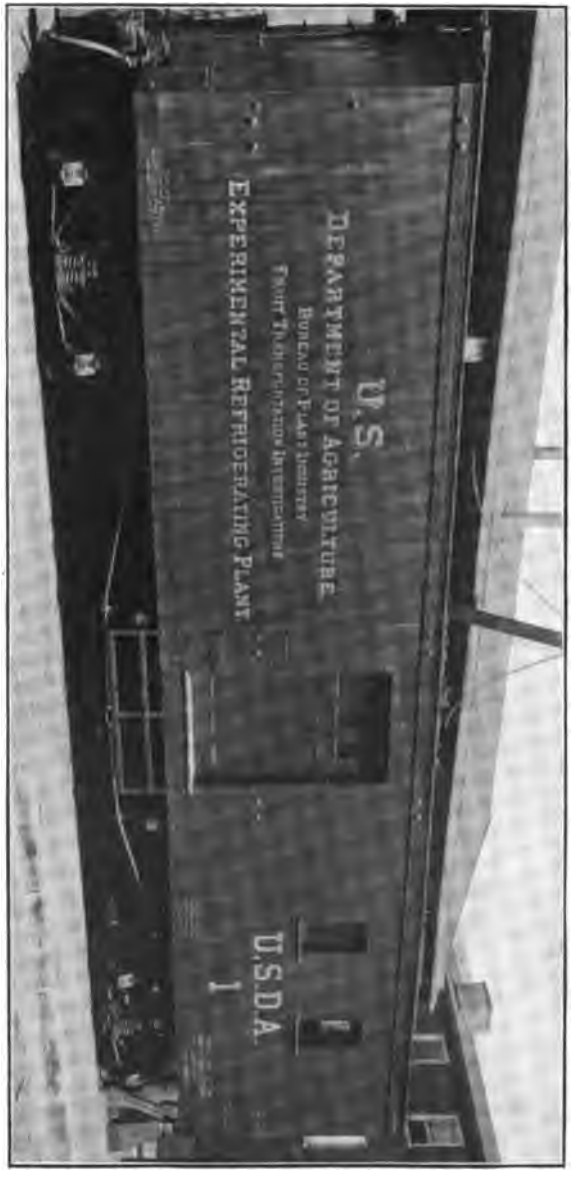
THE PORTABLE REFRIGERATING PLANT OF THE UNITED STATES DEPARTMENT OF AGRICULTURE.

By S. J. Dennis, Refrigerating Engineer, United States Department of Agriculture.

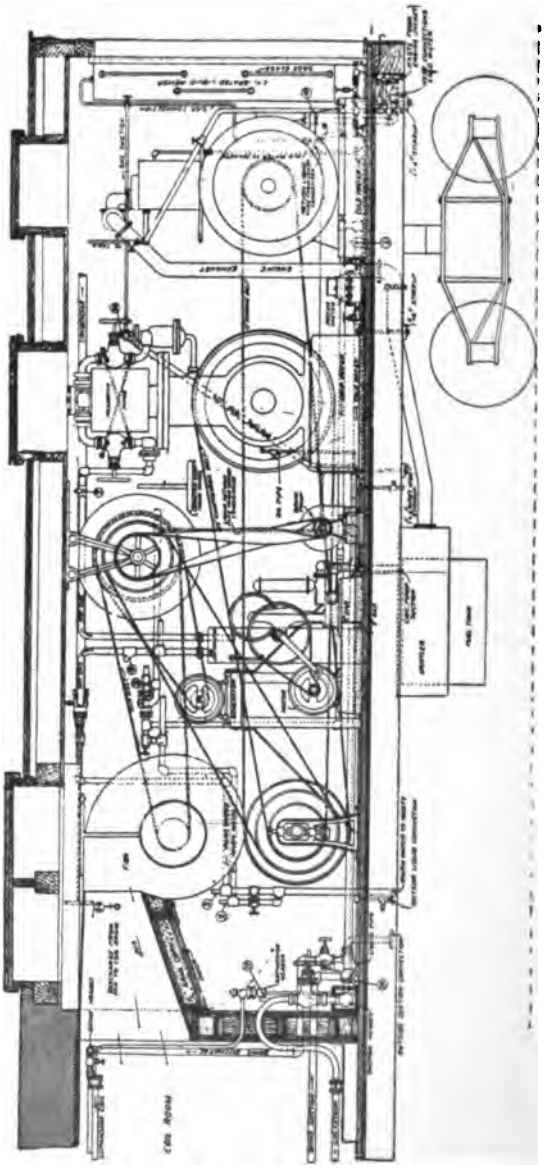
(Non-Member of the Society.)

In the experiments of the precooling of fruit by the United States Department of Agriculture, or the cooling of fruit before shipment, we often found that refrigerating facilities were not available at the points where they were needed for this sort of work, and it became necessary for the Department of Agriculture either to put money into a number of small temporary plants—either small mechanical plants or plants cooled with ice and salt, or to arrange a portable plant on wheels which could be placed wherever we wished to use it, and then after we had finished using it at one point it could be moved elsewhere. We finally decided to build and use a portable plant. The plant is an ammonia compression plant of 12 tons nominal daily refrigerating capacity, and is mounted in a railway box car of the ordinary type, but of special construction to carry this particular plant, the car being built extra strong and heavy in order to afford a stiff platform and support for the machinery. In order to make the plant available for use under all sorts of conditions, it must differ, of course, from the ordinary refrigerating plant, in that the conditions under which it was to be used could not be entirely foreseen. One end of the car is very heavily insulated and carries about 5,000 feet of 1¼-inch expansion pipe, packed into a tank room which occupies less than half the space of an ordinary freight car.

The plant is driven by a 30 horse-power, two-cylinder, vertical kerosene oil engine. The ammonia system consists of a 12-ton, two-cylinder, single-acting ammonia compressor, the whole plant being belt driven. In order to provide a certain amount of control of the refrigeration, a speed governor has been fitted on the engine, and in order to keep the air supply constant a friction drive apparatus is installed on the countershaft in the upper part of the car. From this countershaft are driven a fan and a small dynamo, which runs by this friction drive. The speed of the dynamo can be held constant, while

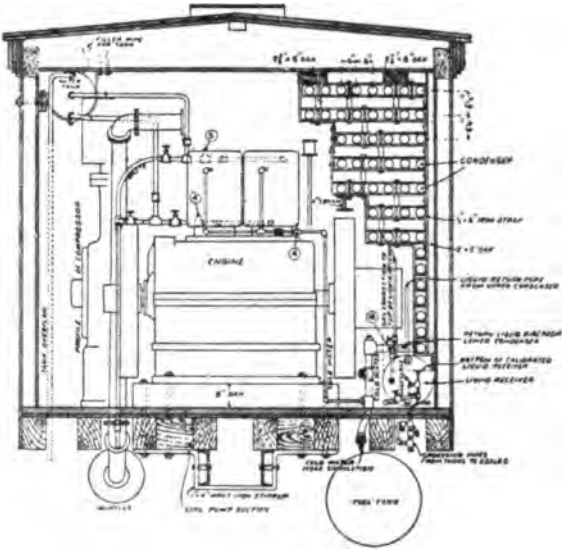


EXTERIOR OF CAR.



PLAN OF INSTALLATION IN CAR.

that of the compressor and the engine may be reduced as necessary. The small dynamo serves to light the interior of the car, and gives light for thermometers located in convenient places, and also serves to drive a small motor for driving the water pump. A small triplex plunger pump is used to pump the water, and has a very large capacity in comparison with the size of the plant, for pumping a large amount of water in case the temperature of the condenser water is unduly high. The pump is motor driven by means of a controlling



End View of Car.

device, in order that the speed may be absolutely independent of the speed of the machine, and the amount of circulating water controlled exactly as required. The condenser is of the double pipe type, and is of very large size, relative to the size of the compressor, in order to allow for warm condensing water, which is, as anticipated, likely to be encountered, especially in the Southwest, where these experiments are likely to be conducted.

In addition, there is also a small centrifugal pump which is intended to be used, if necessary, for circulating calcium chloride brine over the expansion pipe to remove the frost from the pipes and keep the coils working at their best efficiency. The bottom part of

the cooling room is a waterproof tank, and is used to contain the calcium brine.

DISCUSSION.

Louis Block.—How long has this portable plant been in operation?

S. J. Dennis.—The plant is just completed, and it has not been in actual operation as yet, except for the preliminary acceptance tests under the contract.

Louis Block.—Where do you get the water?

S. J. Dennis.—The plant, of course, is dependent on a water supply. It is not intended to be used in transit for refrigerating a train of cars, but only a portable plant such as we can use at different shipping points as we may require in the experimental shipment of fruit.

Madison Cooper.—How is the application of the air to the cooling work arranged?

S. J. Dennis.—There is a 45-inch steel blade exhaust fan, which connects to a scuttle in the roof of the car, having a discharge upon one side and back along the other side, and up through a second scuttle in the roof, the intention being to connect those two scuttles by large insulated sheet iron pipes through the car or storeroom containing fruit to be cooled.

In the construction of such a plant, so very compact, and it also being necessary to so construct the plant that it would stand the jars and bumps incident to shipping it over the railroad, a number of special problems have arisen, and that really constitutes the only claim of this plant to any particular attention. One special feature, which is partly experimental, consists of a large calibrated receiver intended to be used for measuring, approximately, the amount of ammonia being circulated. It is simply a large vertical receiver, provided with the necessary connections, and with ammonia gauge glasses and arrangements for measuring the height of the liquid in the glass tubes, covering the whole length of the cylinder on one tank. I would like to have two tanks, but I could not find room to put in the second. One tank is large enough to run the full capacity of the plant something over half an hour at a time.

There is also a separate receiver under the condensers for taking the condensation direct. I think this covers the main features of this plant. Besides this calibrated receiver, connections are also supplied for a couple of ammonia cylinders to be mounted on scales outside the car, so as to be rid of the vibration of the engine, and

it is the intention to conduct a number of tests, using scales and the calibrated tank at the same time, until the degree of accuracy of this method of using the calibrated tank can be determined, and with the idea of dispensing with the scales so far as possible for much of the work.

Louis Block.—Will this plant precool a carload of oranges in four hours?

S. J. Dennis.—It is not nearly large enough to precool a carload of oranges in four hours.

Louis Block.—In twenty hours?

S. J. Dennis.—It will require about ten to fifteen hours to precool it. It depends altogether on the original temperature of the oranges.

We expect to use it somewhat this winter in California, after working out further details of orange precooling at temperatures which the oranges will stand, and the air temperatures which the oranges will safely stand without damage of one sort or another, and we also expect to use it next year in the South on Southern peaches.

The President.—Has the Government any other plant of that sort, permanently fixed or portable?

S. J. Dennis.—No, there are no other such plants.

The President.—This is the first one?

S. J. Dennis.—This is the first and only plant belonging to the Government.

The President.—Then the Government has been depending upon private individuals for the information they have received?

S. J. Dennis.—We have been obliged to either provide temperature facilities in the way of ice and salt plants, mounted in borrowed refrigerator cars, or to couple up to complete auxiliary plants, as was done in one case in California in connection with the local cold storage plants already erected, and to get along as best we could with the facilities available. But, of course, the conditions of ordinary commercial operation made it difficult for us to get the conditions that we desired, and another thing, the plants already located were almost invariably not conveniently accessible to the fruit shipping districts, and it meant a considerable delay in the handling of fruit to get it to the plant for cooling, and the work already done along these lines has shown that the element of time in cooling, if precooling is to be applied, is important. The cooling should be done as quickly as possible, because one day at the highest temper-

ature means many days' difference in the life of the fruit after it is cooled to a lower temperature, and in order to work out conclusively the value of precooling in extending the time which such produce can be held it was necessary to apply the refrigeration very promptly.

Louis Block.—At our meeting last year someone from the Agricultural Department told us that precooling was of the greatest possible importance. Now this morning Mr. Powell said that precooling apparently was not of such importance, but that handling was of the greatest possible importance. Now, has it been definitely ascertained whether precooling is of the greatest advantage or whether it is the handling of the fruit?

S. J. Dennis.—Mr. Powell's statement related principally to the citrus fruits, oranges and lemons.

Louis Block.—That is what I refer to.

S. J. Dennis.—The orange and the lemon are slow ripening fruits. They do not absolutely need refrigeration to give them long life or good carrying capacity in shipping long distances, provided the fruit is absolutely sound. Oranges which are quite eatable in early spring can hang for a couple of months on the trees and continue to ripen very slowly and still be entirely eatable a couple of months later, which is a very different proposition from some quicker ripening fruits. But as regards oranges, precooling is not absolutely necessary provided methods of handling can be adopted which will insure the orange being placed in the package in absolutely sound condition. Under present conditions, however, which obtain in California in the lowest grades of packing houses, that is not realized, and many oranges are damaged very slightly, enough to make them susceptible to mould, and the precooling is being adopted very largely to counteract the effect of such slight injury. In the winter time, during the cold weather, all orange shipments are sent through under ventilation, without any ice or refrigeration of any sort; that is, except the natural refrigeration that they get by the cold air circulating through the ventilators of the cars, which are ordinarily left open. In the beginning of the season, when the fruit is shipped rather green, the ventilators are only closed to keep the fruit warm, and allow it to ripen more or less on its way across the continent. Later in the spring the cars are iced and the ventilators opened, and it is at that time of the year that the precooling becomes of special value, but during the winter season the fruit is often naturally precooled, so to speak, and needs no artificial refrigeration of any sort.

George Berna.—What temperature will oranges stand before freezing?

S. J. Dennis.—That has not been determined. That is one of the problems which I shall give attention to during the coming winter.

No. 61.

Discussion of the Topic—WHAT ARE THE THEORETICAL AND PRACTICAL LIMITATIONS FOR THE AMOUNT OF ICE PRODUCED PER POUND OF COAL IN DISTILLED WATER CAN PLANTS?

Louis Block.—My experience has been that 10 per cent. more ice in weight can be manufactured than the boiler can evaporate per ton of coal burned. That is, if a boiler evaporates 10 tons of water to 1 ton of coal burned, 11 tons of ice can be manufactured by having an evaporator, a single-effect evaporator, in the exhaust pipe. Practically, when coal is weighed for four summer months in the year, and ice is weighed for the same length of time, the product is considerably less. It drops 12 to 15 per cent. from that which is possible on a test. I have several cases in my mind, one at the present moment. The plant is making 35 tons of ice with 5,400 pounds of bituminous coal. I do not know what the heat value of that coal is; I only know the fact that the coal is weighed every day, and so is the ice. This plant is operating now with an ammonia condenser pressure of 110 pounds, and a suction pressure of 20 pounds. I have another case in my mind where, weighing the coal for four months in the year, and weighing the ice for five months in the year, the plant manufactured 7¼ tons per ton of coal burned for May and June, while for three months—in fact, for July, August and September—it was only 6½ tons of ice per ton of coal. This coal, however, only evaporated 6½ pounds of water per pound of coal burned.

I have another plant in my mind where 11¾ tons of distilled water ice were made with New River coal, which evaporated close to 11 pounds of water per pound of coal burned. It depends, of course, largely upon the condition of the plant and how it is operated. When I refer to a possibility I mean something which can be reached by an expert who subjects the plant to a test of a week or ten days. When the plant is in the hands of an operating engineer, and who sometimes of a Sunday, because his ice storage room is full, or over night, because the ice sales fall off, is compelled

to shut down his plant, the reduction will be from 12 to 15 per cent. from that which is possible.

Thomas Shipley.—My observation and experience have shown me that the usual cause of low economy in the ice plants making distilled water ice comes from the auxiliaries used. Of course we all know that the boiler plant is the primary factor in economy of any steam plant, but it is not the boiler plant we are discussing. We are assuming that the boiler plant is up to the usual good standard, and is able to evaporate 10 pounds of water from and at 212° per pound of best steam coal. To get at the possible economy of compression ice-making plants, such as we are discussing, we must know the economy, not only of the engine driving the ammonia compressors but also the economy of the auxiliaries.

For example, let us take a 100-ton ice plant. It requires about 220 horse power to drive the ammonia compressors of such a plant, and it requires about 30 horse power to drive the auxiliaries. This does not include the power required to furnish and distribute the condensing water. The power required for this purpose varies so much that it is impossible to consider it in a standard calculation. It must be taken into account separately for each plant.

The fuel economy of the plant will depend upon the type of engines that are used to produce the power required. If a simple Corliss engine is used to drive the compressors and the ammonia-condensing pressure is about 165 pounds, about 80 tons of steam will be required for this work, figuring 30 pounds of steam per engine horse power.

If we use electric motors to drive our auxiliaries, and drive the electric generator by a high-speed automatic engine, we would require about 14 tons of steam, figuring 35 pounds of steam per engine horse power. This gives a total of 94 tons of steam to produce the power necessary to operate the plant. Allowing 10 per cent. loss of distilled water, we would require 110 tons of distilled water to produce 100 tons of ice, or 16 tons more than required to drive the engines operating the plant. If this extra 16 tons of water is made up from steam coming directly from the boilers, the economy of the plant would depend entirely upon the economy of the boilers. Hence, assuming that the boilers evaporate 10 to 1, the plant would produce about 9 pounds of ice per pound of coal.

If, however, the additional 16 tons of water are produced by an evaporating system, the economy of the plant would depend, first, upon the amount of steam used for power, and, second, upon the

economy of the evaporating plant producing the additional water required.

It is safe to say that each effect of an evaporating plant will produce three-quarters of a pound of vapor for every pound of vapor entering the effect; that is, a single effect will produce 1¾ pounds of distillate for every pound of steam entering the effect. A double effect will produce about 2¼ pounds of distillate.

If an evaporating system be used which will give 4 pounds of distillate per pound of steam entering the first effect, it would require but .4 of a ton of coal to produce the 16 tons of additional water. This, added to the 9.4 tons needed to produce the steam used for power, would give 9.8 tons of coal to operate the entire plant, or more than 10 tons of ice per ton of coal.

If the engines were compound condensing, the steam needed for power purposes could be reduced to 50 tons total. This would leave 60 tons of water to be obtained from other sources. If this water was obtained from an evaporating system giving 4 pounds of distillate per pound of steam, the total steam used for the boilers would be 65 tons, which, figuring at 10 to 1 boiler evaporation, would mean 6½ tons of coal for the 100 tons of ice, or 15.3 pounds of ice per pound of coal.

Even better economy than this can be obtained by putting in more effects in the evaporating system.

Another economical method for obtaining the additional distilled water needed is one where the feed water is circulated through a live-steam exchanger. This heats the feed water to very nearly the temperature of the steam in the boilers, while the steam which does the heating is condensed and used to swell the amount of distilled water to be used to make ice.

This last method is one that is being used on a plant at Rocky Mount, N. C. This plant has a compression and an absorption machine, and it has created considerable newspaper talk because of the amount of ice it is said to produce per pound of coal. This heat exchanger is the only thing about the plant which is in any way out of the ordinary, and it is to this apparatus the good economy of the plant is due.

The absorption plant was put in last, and the only improvement that I could find, except the exchanger referred to, was to abandon a number of water pumps which were used to pump condensing water from a number of wells on the premises, and to use an engine to drive a centrifugal pump which circulated the condensing water

over the cooling tower; or, in effect, the auxiliaries were made more economical.

Now, as to the practical possibilities, this depends entirely upon whether it is a good investment to spend a lot of money to save coal. It is a question of dollars and cents. It depends upon the cost of the coal. You can figure out your coal consumption and find out what it has cost you for coal per ton of ice produced. It may be possible for you to reduce the cost of coal per ton of ice by investing 15 or 25 per cent. more in the cost of your plant, but it is not always economy to do this, especially if you are getting reasonably good economy from your plant.

For instance, if your coal cost you $3.20 per ton, and you get 8 to 1, the coal cost would be 40 cents per ton of ice. If you added apparatus to your plant so as to get 10 to 1, your fuel cost would be 32 cents, or a saving of 8 cents per ton of ice. If you added further to your plant and got 12 to 1, the fuel cost would be 26⅔ cents, or an additional saving of 5⅓ cents.

Now, when you deduct the extra cost of operation, and the extra interest on the investment, you will find the net saving so low that it is a question if it pays to put the extra cost into the plant. If coal costs are high, then the extra cost for the high coal economy is justified, but otherwise it is better practice to put in a plant with a good first-class boiler system, and use simple Corliss engines to drive the compressors, and put in economically driven auxiliaries so as to bring the steam consumed for power to a point where it is less than the amount required to produce the needed water to fill the cans.

It is false economy, and very bad practice, to put in a plant where the auxiliaries are not driven economically. This practice often explains the failure of ice plants as money makers.

Henry Torrance, Jr.—Mr. Shipley speaks about evaporating 10 pounds of steam from 1 pound of coal, and getting 9 to 1 under actual conditions, thus allowing for a loss of 10 per cent., which I claim is too small for a compression plant, because the cylinder condensation is about 20 per cent., which goes off in the drip from the oil separators.

As Mr. Shipley has told you, in our absorption system we heat the feed water with live steam up to the temperature of the boiler, say 300°, and thus have been getting an evaporation 10 per cent. better than others.

The cylinder condensation of our pumps is pretty low, because there is more or less back pressure. In fact a little back pressure

on the pumps does not do any harm, as long as you do not have too much steam for the generator. If we thus reduce the cylinder condensation down to the minimum, the total loss, including reboiling and 3 per cent. meltage in the cans, would be about 10 per cent.; that is about as low as you can get, against 25 per cent. for a compression system. Deducting this loss from a boiler evaporation of 10 to 1, you get the practical limit of what a distilled water plant can do, viz.: 9 to 1 for absorption and 7.5 to 1 for compression. I want to say further, as to boilers, that the horizontal tubular boiler, run with a low temperature of flue gases, will do just about as well as any other boiler. George H. Barrus tested all kinds of boilers, and he found that the principal condition governing economy was the temperature of the flue gases. Now, if we take the ordinary boiler that is running on some system not very economical, and add a system that is economical, the capacity of the boiler required is very much less, and usually we get additional economy in the boiler, besides an improvement in the machinery.

R. L. Shipman.—Along this line there is 10 or 20 per cent. of loss of condensed steam due to the oil in the condensed steam. It seems to me it might be worth while for some of these manufacturers to look into the possibility of graphite lubrication for the engine in an ice-making plant. Recently I have been operating an engine with dry graphite lubrication. The engine has been operating about fifteen weeks, three hours a day. I have not put a drop of oil in the engine; it is operated entirely by dry graphite lubrication, and in that way this blow-off of 10 per cent. that has been lost might be saved, because the graphite would very easily filter out of the condensed steam.

Thomas Shipley.—I would like to answer Mr. Torrance a little bit in connection with this comparison between absorption and compression. I want to say that the plant which I saw down at Rocky Mount, N. C., had a Chandler-Taylor engine exhausting into the generator, and lots of oil was used on the engine. There was also a weak aqua pump. This pump required oil to be used in its cylinder, and this oil went into the generator, and there were several other pumps exhausting into the generator. In fact, all the pumps and engines used to operate the plant exhausted into the generator, and all used oil in their steam cylinders. There is no difference in the oil required to operate a pump or an engine, and there would be just as much oil in the exhaust from them going into a generator as there would be if the exhaust went into a condenser; hence it cannot be said that the distilled water coming from an absorption machine.

which is operated from exhaust steam, is more free from oil than the distilled water coming from a condenser of a compression plant.

Louis Block.—Both Mr. Torrance and Mr. Shipley said that 10 per cent. of steam would be lost from the boiler to the engine. The loss is very much greater than 10 per cent. Steam cylinder condensation will amount all the way from 20 to 40 per cent. With a good Corliss engine and very high pressure steam it may be kept down to 25 per cent. We do not recover any of the drips. They all go to waste. The ice which we make and melt out afterward amounts to at least 30 per cent. The amount which we skim off the skimmer or reboiler amounts to at least 2 per cent. So that I think it is very conservative to estimate a loss of 25 per cent. of all the steam which the boiler sends to the engine.

R. L. Shipman.—Mr. Block has placed this cylinder condensation abnormally high. In a great many tests of steam engines I find it to run, by testing the quality of the steam on leaving the cylinder, not more than 10 per cent., and commonly less. It must be remembered that cylinder condensation may run from 20 to 25 per cent., but we have re-evaporation from expansion, which evaporates a large percentage of that which was condensed during admission.

Louis Block.—I refer especially to Chief Engineer Isherwood's experiments which were made thirty or thirty-five years ago, and he positively and definitely determined what the steam cylinder condensation in Corliss and in other engines was. I have not heard of anything since then which has cut that down.

Henry Torrance, Jr.—The question all depends, I understand, on the cut-off. If you run a Corliss engine at an economical point and short cut-off the condensation will be about 20 per cent., while with a small engine exhausting into our absorption generator without cut-off the condensation is scarcely 5 per cent. Then there are 3 per cent. loss in meltage at the ice cans, 2 per cent. on the reboiler, and 3½ per cent. condensation from auxiliary engine (this engine uses about 70 per cent. of the total steam, and has a condensation of 5 per cent.; 5 per cent. of 70 per cent. equals 3½ per cent. total loss), and add all those together and you have about 10 per cent. total loss in a well-designed absorption system.

D. S. Jacobus.—Mr. Block's figures for condensation are for the amounts present in the cylinder of an engine at the point of cut-off, or near the end of expansion, as determined from the indicator diagrams. The amount of condensation in the exhaust steam is not as large as the amounts computed from the indicator

diagrams, on account of the re-evaporation of some of the moisture when the steam is being exhausted from the cylinder. If the heat equivalent of the work done by the engine is taken from the total heat in the entering steam, the remainder will be the heat in the exhaust, except for the amount of heat which is radiated, and when computed in this way the condensation is less than that which occurs in the cylinder.

Louis Block.—You did not mention the initial steam cylinder condensation. Condensation generally takes place the moment the steam enters the cool cylinder, and that condensation must be added to the cylinder condensation which is due to the work done.

Henry Torrance, Jr.—I understand Mr. Block is talking about compression refrigerating machines, and I think he is right, but I am talking about absorption machines.

O. J. Morris.—It may be interesting to some of the gentlemen here to know that in the Southwest this question is very important and an all-absorbing one with the ice manufacturer, for the reason that we pay very high in that section for coal and the water is bad, and we do not get any results in that country with the compression system or any other system that I know of that I have heard talked about here. For instance, I have tabulated reports in my office from 100 different plants operating in the Southwest, and I find that we are getting only about 4 tons of ice for 1 ton of coal consumed—the coal that we get through that part of the country. I find that our people know very little about the proper way to handle coal or to buy it, and a good many of them have adopted the rule of putting a meter on the feed water, measuring the water and weighing the coal, and when they find they get 8 to 1 evaporated they think they have done remarkably well. We are only getting an average evaporation through that part of the country of about 8 or 8½ pounds.

Louis Block.—The McAlister coal is perhaps the best and costs $6.50 a ton. A large quantity of lignite coal is burned, which will give you about 4 to 1, so that I should not wonder at all if your average results for ice were only 4 to 1, but that is due to the inferior sort of coal which you are burning.

Charles Dickerman.—I would like to ask Mr. Block or Mr. Shipley, or any other of the gentlemen who have compiled figures, as to what we actually do get in the average can plant with single effect. If anyone has compiled figures regarding a number of plants —not one plant or two or more in any one location, but an average which will cover, say, the Eastern part of the country (which are

the conditions we work under)—I would like to know what we do actually get out of a single effect can plant?

Louis Block.—What do you call a single effect can plant?

Charles Dickerman.—Single evaporation; not multiple effect evaporation. I mean a single expansion Corliss engine and a single effect steam condenser and the usual auxiliaries required in an ice plant. What do the best of them made turn out, that is, as an average?

Thomas Shipley.—Mr. Dickerman, I will say that the York Manufacturing Company owns a plant in Philadelphia. It averages about 6 to 1 the year round. That is not a test run, but the coal we paid for and the ice we made. This plant is a compression can system. We put in an absorption plant at Asbury Park two or three years ago. It is a plate plant. This plant made 9 to 1 for 135 days during the summer this year.

No. 62.

Discussion of the Topic—WHAT ARE THE BEST TEMPERATURES, ACCORDING TO MODERN PRACTICE, FOR HOLDING EGGS, BUTTER, CHEESE, MEATS AND FRUITS IN COLD STORAGE WAREHOUSES?

Homer McDaniel.—The question as to what is the proper temperature for all these various items has been largely a matter of experiment by the operators of the various cold storage warehouses, and I rather think that the results of these experiments, so far as some of the items are concerned, have brought our ideas close together. For instance, on eggs, I think it is understood generally that about 31°, or from 30° to 32°, is the right temperature. On butter, the stand that various houses take varies considerably. I think that good butter, when first placed in storage, and as soon after making as possible, should be carried at a sharp temperature —in the neighborhood of zero. Toward the end of the season, or at the time the butter is moved, I always favor letting the temperature come up, say to 16° and 20°. We do not always do that, because our customers do not approve of it, but I really believe that brings the best results to the consumer.

For a number of years cheese was carried by most houses in the neighborhood of 38°, 36° and as high as 40°. Recent experiments by the Government have, I think, led most houses to carry the most of their cheese at a temperature about the same as eggs, say 31° or 32°.

Meats, of course, depend upon what the customers want. If the meats are wanted frozen the sharp temperatures in the neighborhood of zero are best when they are first placed in the warehouse, after which they can be placed in holding rooms at a higher temperature.

In fruits there is a large field; except as to apples, the storage is not heavy. Apples are carried close to 30° or even 29°. I think that rather covers my ideas, so far as the items mentioned go.

C. H. Parsons.—From what little experience I have had I would agree with Mr. McDaniel on most of those temperatures: although in the case of butter, while he may be right about raising the temperatures along the latter part of the storage period from

zero up to 15° or 20°, we have never done that, and have never felt that it was wise to do it. It is my belief that while there is a great trade demand for zero temperatures for butter, in some cases even less than zero, higher temperatures would be fully as sensible, say from 5° to 8° above zero; as I believe that cleanliness is an all-important item in the carrying of butter—cleanliness, purity of the atmosphere in the room. I think it has fully as much to do with a fine keeping of butter as very low temperatures.

We carry eggs at about 29°. In the case of apples, we have never attempted to carry them as low as Mr. McDaniel, who says he carried his at 29°; our temperatures on apples running from 32° to 32½° or 33°. We have about the same temperatures on cheese, perhaps a little less, as we do on the apples. In freezing meats we attempt to freeze them in a very sharp freezer, and freeze them quickly and continue them at low temperatures.

Eugene W. Lewis.—As regards the temperature of eggs, in 1893, by an accident to a thermometer which was wrong, I discovered that 29° was about the temperature at which to carry them, and I have been carrying them ever since at that temperature; in fact, have had rooms for four months that were 28°. Apples, Baldwins and apples of the hardier nature, we have carried as low as 30°. The fall fruit, which has a larger percentage of water, we have carried at 32° or 33°. I have carried cheese at as low as 29°. Two years ago I had a Canadian cheese expert come in our plant at Jersey City and he asked at what temperature I was carrying cheese, and I told him 30°. He said, "My dear boy, you are wrong." I showed him five cheeses that had laid in a corner of a room for eighteen months, at no time at a higher temperature than 30°. I had him sample the cheese, and he said it was fine. In fact, we carried a good many million pounds at 30°, never higher.

I don't think there is any doubt as to zero temperature having advantages in the keeping of butter. Of that I am satisfied. Still, from the cold storage standpoint I would like to see it at 20°, because it costs less money to carry. I think the United States Department of Agriculture experts made tests three years ago in Booth's cold storage warehouses in Chicago; and when they allowed a rise of temperature after two or three months the butter became "fishy" on the bottom and sides and top, and they decided that anywhere from zero to 5° below or 5° above was the best. Two years ago we had butter that had been carried on an average not higher than 5°, and it was taken over to the store of a prominent commission merchant after it had been stored in Jersey City for a year and a half—I do not object to giving their name—James Row-

land & Co. Thirty tubs of that butter were sold from their floor as against June butter of that year. So I do not think there is any question, together with the experiment of the Agricultural Department, but that zero is the proper temperature for butter.

We carry meats at anywhere from 5° to 10°, and let the temperature run up to 20°, because the evaporation per pound is less at 20° than it is at 5° above zero, and it does not seem to hurt the meats any. We carried in the Jersey City house three and a half million pounds one year.

I believe this covers the goods that were mentioned. As regards fruits, we have had the biggest success with Georgia peaches at about 35°. The smaller fruits, of course, we only carried for twenty-four hours, something like that, so the temperature has not been considered. Celery is carried at about 33°, and I believe we had as good results as any plant in the world on celery. We carried this year over seventy-five cars.

Howard L. Jenks.—I heard a discussion at a meeting of warehouse men two years ago, and they had it figured out nicely how much butter would depreciate in percentage month after month, so that after a year or so it had lost quite considerably in its value. I have just heard you say that butter which was stored a year sold better than fresh June butter, and compared favorably.

Eugene W. Lewis.—I think the Government report on that test of butter was that the butter had scored 90. That is, this scoring is for flavor, salt, color and body, and in six months it only went off three points; while that which was carried 15° above zero went off something like seven or eight points from the original score.

Louis Block.—In that case, then, the butter which you mentioned had deteriorated 7 points?

Eugene W. Lewis.—I did not see it scored. It was not tested. It was simply a commercial proposition, where a buyer took butter that was eighteen months old against butter that was eight or nine months in storage.

Louis Block.—Depending upon his own test?

Eugene W. Lewis.—Yes; his judgment when he bought it, at least.

Louis Block.—I want to know about eggs. The question of the storage of eggs is often sprung upon me.

Eugene W. Lewis.—Take eggs from the northern part of Michigan or Indiana, or even Iowa, and after four to six months in cold storage I defy anyone to tell whether they are fresh or cold storage eggs. After that time the air space in the upper part of the shell becomes greater, and black rings form inside, which shows it

is a cold storage egg; and then again it has absorbed the odor of the cardboard in which it is packed. I have eaten and given others eggs that were six months in cold storage, and others a few weeks old, and none of us could tell the difference. On September 8 last we had a car of eggs come in from Michigan—fresh gathered Michigan September eggs. I looked at them and I brought down some of this same man's eggs that were put in there in the latter part of April, and the April eggs were better in every sense, commercially or any way you want to take them; because the September eggs had takn anywhere from one to four weeks to gather, the farmer holding for a higher market, in all kinds of changes of temperature.

Henry Torrance, Jr.—But suppose you compare a five months' egg with an egg from the hennery?

Eugene W. Lewis.—You couldn't tell the difference.

Theodore Kolischer.—In connection with the International Congress of Refrigeration in Paris, one of our delegates had prepared a very excellent paper, with lantern slides, tables, etc., about the researches on chickens in cold storage. The reading of that paper was listened to by several hundreds of the members of that congress. It was quite the sensation of the congress. In that paper describing the researches and so forth, the delegate conveyed, probably unintentionally, the impression that the practice in America was to store chickens three and four years in cold storage houses, because the delegate had examined some of the chickens that were in some cold storage house for a period of three or four years. I therefore embrace this opportunity, where there are so many gentlemen connected with cold storage houses, fairly conversant with all the practice, to ask if it is the custom in the United States to keep chickens and poultry as long as three or four years; because it made quite an unfavorable impression upon the foreign delegates that we were using cold storage to preserve perishables for such an enormous length of time.

Eugene W. Lewis.—I would say that the charges for storage for thirty-six months would amount to half of the original cost. That makes it prohibitive, from a commercial standpoint. There are not many chickens carried that long, because of the evaporation or shrinkage. They would taste like so much sawdust. That is so. Deer and bear meat have been carried, and I have had them five years in Chicago. I didn't eat any of the meat, however.

G. Harold Powell.—I think the nearer you can get the temperature to the freezing point of most fruits the safer it is for carrying quality, but there is a great deal more difference in the keeping

quality of fruit brought about by the method of handling and packing the fruit before it goes into the warehouse than there is by a difference of two or three degrees of temperature after it reaches the warehouse. To protect against loss caused by the improper handling it may be subjected to before it reaches the warehouse it is generally necessary to store fruit at the lowest safe temperature. Fruit stored quickly and handled properly may be as safe in a 40° temperature as it would be in 32°, if there has been a delay of ten days before it reaches the warehouse. You would probably get better results from 31° to 32° with a quick-ripening summer or fall apple right from the trees, as the ripening needs checking quickly. Citrus fruits and the apple, pear, peach and small fruits should be held at the lowest possible temperature without freezing. We have had no bad effects from holding peaches at a temperature of 31° or 32°. I think Mr. Lewis says 35°. We have taken peaches from the same trees and packed them at a temperature of 31° to 36°, and had the flesh discolor much more quickly in 36° than in 31°. The peach is a bad product in cold storage, except for a few days. Two weeks is the longest Eastern peaches should be stored. I never saw a peach in storage longer than two weeks that retained its quality, though it may have been normal in appearance. We have been making a test this last year of holding oranges at different temperatures, and we find that 32° gives better results for long storage. Oranges are an uncertain product after three or four weeks in storage. They keep well, in good physical condition, for three or four weeks at 32°, and sometimes in higher temperatures. The skin may show a brownish discoloration, and several other obscure troubles may develop. I think that is as much as I care to say on the temperature question. We have never had facilities by which we could compare temperatures accurately, and, as I say, I do not feel that a difference of a degree or two of temperature in the warehouse is as important in storage work as it is to handle the fruit carefully and store it quickly after it is harvested.

Eugene W. Lewis.—Do I understand you to say that you would carry lemons for three weeks at 31°?

G. Harold Powell.—No, not lemons. I referred to oranges. We have stored lemons for a month or more at 32°. There is no object in holding lemons in cold storage. Lemons handled properly can be kept for months in common storage. If a lemon rots it is a sign it has not been handled properly.

Louis Sterne.—Mr. President, we have not storage of fruit and produce, such as is mentioned here, in Great Britain. Our principal

storage is frozen sheep, lambs and beef. Those are what we know most about. They come frozen and remain frozen until taken to the market for consumption. But as to fruit, we have some interested in this business; still I think there is scarcely a single cold storage warehouse devoted to fruit. There is a great deal of butter and cheese kept at a lower temperature than these gentlemen mention. Butter remains frozen from the time it is placed on board the ships in either Australia or the Argentine Republic, and it remains in that condition until it reaches the retailer's hands. We have very little fruit in storage; none to warrant any suggestion to the gentlemen present.

Irving Warner.—Will you tell us what are the temperatures of the holding rooms in London for the frozen meat after it arrives?

Louis Sterne.—From 17° to 18° Fahr.—not above 20°.

No. 63.

Discussion of the Topic—WHEN STORING EGGS, BUTTER, CHEESE, MEATS AND FRUIT IN COLD STORAGE WAREHOUSES, IS IT BETTER TO USE A FORCED AIR CIRCULATION OR TO PIPE THE ROOMS?

Madison Cooper.—The storage of eggs requires air circulation from my standpoint; the storage of butter does not. The same comparison might be applied to meats and fruits. Meats do not require air circulation, but fruits do. These are rough statements, but they cover the general idea. The questions of length of time in storage, and how the air circulation is applied, have much to do with the proposition. I have my own particular method of air circulation, which, of course, I advocate and consider superior to most of the other methods. The direct piping of rooms is useful and advisable, and almost necessary where the rooms are small, for small purposes, and air circulation is out of the question because the expense of operating is too great.

Louis Block.—Mr. Cooper, would you advocate air circulation for eggs and fruit all the time or only shortly after they have been put in storage?

Madison Cooper.—The air circulation should be maintained continuously, without any stoppage, for the whole period of storage.

Louis Block.—Do you circulate in the room itself or from a pipe chamber down to the room, and then up to the chamber again?

Madison Cooper.—From a pipe chamber or coil room.

Louis Block.—Does each room to be cooled have its separate pipe chamber and separate air pipes?

Madison Cooper.—Yes, that is the best arrangement, but it need not necessarily be so arranged.

Louis Block.—So that you do not get lemon flavor into the eggs from lemons stored in another part of the building?

Madison Cooper.—Yes, lemons should be stored in a separate building or be handled through a separate outside entrance.

Thomas Shipley.—How about onions?

Madison Cooper.—Onions are the same.

Answering the questions as to having a separate coil room or coil loft for each room, that is not necessary where the same product is stored in different rooms, because you can have as many rooms as you want on one air-circulating system as long as the product is the same in the various rooms. Separate goods, different classes of goods, should have a separate air-circulating system. In connection with the storage of butter with air-circulating systems I know of one large house in Chicago where they use an air-circulating system for butter storage and butter freezing, with both temperatures below zero. I have designed one house where the air-circulating system for butter and freezer storage works at 7° to 10° below zero. I regard air circulation as unnecessary for freezer purposes at that temperature, and in this case I used it only as a matter of mechanical practicability. It was easier to do that than to pipe the rooms direct.

John E. Starr.—I might add, Mr. President, that I have had to do with several plants for the distribution of refrigeration by what is known as the indirect or fan system. I may say, I think, without fear of contradiction, that probably 80 to 85 per cent. of all the eggs which are held in this country are held under what is usually termed forced circulation. The term forced circulation, however, it seems to me, is susceptible of being greatly misunderstood. The general idea that a layman or even a man somewhat acquainted with refrigerating matters and cold storage matters is very apt to get when you use the term forced circulation is the idea of a blast of air, of a rapid current of air going through the rooms refrigerated. There is no question in the mind, I think, of any man present but that it is necessary to have some circulation in any room. Even in a room which is piped on the side there is a circulation, of course, by reason of the air touching the side pipes, dropping down, circulating across the room, coming back on the pipes, and so turning over and over again in the room.

Now the advantage of the fan system is simply this: That by the use of such a system the rate of circulation, the speed necessary, can be regulated to a nicety; also that the air can be distributed to a nicety by means of ducts and dampers throughout the room. It has been thought in a great many cases, for example, where extremely low temperatures are used, that it is simply necessary to dump the air into one end of the room and pull it out at the other; because it is argued that it does not make very much difference, when you are down to temperatures of zero or 5° to 10° above, whether the air is a degree or so different in one end of the room

from the other. This, I think, is rather a false position to take, for this reason: that in order to refrigerate a room at all a certain amount of air must have to pass through it and must have a certain range of temperature from the incoming temperature to the outgoing temperature. When a room is full of eggs there are, of course, gangways, passageways, for piling and handling of the eggs. If the room is reasonably full and the air is admitted at one end and taken out at the other end it necessarily follows that the speed of the air down these passageways is going to be very rapid. It is not the speed of the air, which would be equivalent to the speed of a cross-section of the room, but it is a comparatively narrow passage; and in some cases this might lead to the drying out of the eggs, no doubt drying out the eggs which are placed in the passageways. I think that if it is admitted that any circulation at all is to be used in a room—and I think it must be admitted that some circulation is absolutely necessary, otherwise one part of the room would be at one temperature and another part of the room would be at another temperature—I think it follows that a duct system, properly distributed, properly letting in the air at various points—not all dumped in at one point, but admitted all along the sides, all along each side and then brought out in the centre, or in a large room perhaps a great subdivision of ducts—that the entire temperature could be held exactly even, and that such a room, with certain kinds of goods, is a very necessary and almost an absolute requirement for success in storing such goods.

Nelson J. Waite.—I find that there is a wide difference on this point in the cold storage business, and I also find the same divergence of opinion among our customers. Some who have acquainted themselves with the different methods of refrigeration advocate the circulation of air and others find objection to it. In our part of the country we have found that so to a large extent, and have rather taken the middle ground and installed both systems, so to speak, by piping the rooms for direct expansion; and of course objections to that are well known, and the dry box and the fan system were installed to counteract those. Yet at the same time it is not intended to be used continuously. With the various products that we are called upon to handle it gives a little more flexibility to the general warehouse in this way: it is not strictly necessary to have the rooms in groups controlled by separate fans, so as not to have the different classes of goods intermingled and subject to the different odors and the like of that. In using a pipe system, of course, more care has to be exercised to pipe the rooms properly to get a uniform

distribution of pipe surface as near as possible, and by that means obtain a uniform temperature. By so doing, and with the aid of the customary fan system, I think that we get far better results for some classes of goods, and so far most of our customers seem to be well pleased with the results.

Gardner T. Voorhees.—Mr. President, the Quincy Market Cold Storage and Warehouse Company, Boston, Mass., has probably the largest or at least one of the largest capacities under air circulation of any plant in the country. When I was with it we went into this subject at that time pretty thoroughly, and I had, in connection with Mr. Stoddard, quite a good deal of experience on that, and have had some since, and these points were much impressed on me at the time and have since been more impressed on me—that you can carry a more even temperature throughout the room with air circulation than with direct pipes. If you have pipes you have got to be careful with eggs and goods, not to put them too near the pipes. The radiation that comes from the pipes will oftentimes affect goods that are too near the pipes; whereas with a circulation of air you do not have that to contend with. Then you have the question of the drip to take care of in the rooms where the pipes are; that is, the question of the drip getting loose sometimes and wetting goods or packages below or on the floor. It is almost a certainty that it is easier to control the humidity with air circulation than with pipes. That is, you can make the air wetter or drier, as you may desire. There is no question but that if a storage house is built of wood, with wood sheathing inside the rooms, the sheathing will keep dry and in better condition with air circulation than it will with pipes in the room. If there are pipes the ceilings and walls naturally have a colder temperature than the air, and they condense moisture on them; while with air circulation the ceilings and walls are always warmer than the room, and you do not have condensation on them.

Another great point is that pipe in the bunker room will transmit about twice as much heat with the same temperature of brine and the same temperature of air for a given amount of pipe as it will in the storage room. Another advantage is that in case you are putting in fruit you can concentrate all the cooling energy of your bunker room on the fresh room of fruit, and by this process, you might say, almost the whole service of the whole warehouse can be concentrated there; whereas you will not be able to do that where each individual room is piped.

Louis Block.—Did you say, Mr. Voorhees, that the pipe service is twice as efficient?

Gardner T. Voorhees.—The pipe service, from experiments made very carefully by me, repeated time and time again, demonstrates that which will be worth making a note of—that the pipe will transmit twice as much heat with a given temperature of the air and brine in the bunker room as if it is put on the walls as usual.

Louis Block.—It would depend largely upon the speed of the air, would it not?

Gardner T. Voorhees.—Now you are getting to the reasons for it. The ordinary speed of air with coils as ordinarily installed in the bunker room causes the air to go through the bunker room pretty rapidly. The coils are pretty close together. Under those conditions pipe surface is twice as valuable as if on the walls or ceilings of a room. So you only put one-half the quantity of pipe in your bunker room as you would if you put it on the walls in the old way.

Louis Block.—How many cubic feet of space has the Merchants Refrigerating Company, Mr. Greene?

V. R. H. Greene.—It has 4,300,000 cubic feet of space.

Louis Block.—You have no air circulation?

V. R. H. Greene.—We have no air circulation. We think it is a debatable question. We acknowledge that to keep eggs in good condition you must have circulation, but it need not necessarily be derived by means of fans. The secret of carrying eggs successfully is to carry a condition of moisture so that they will not shrink excessively and at the same time will not form mould. That can be derived—or at least we do over in our Jersey City house—by having two brine systems, a high and a low brine. Originally we had a low brine. That meant we had very little service pipe. We had to carry a very sluggish circulation and kept the room very dry, a condition which was very unsatisfactory. Then we debated whether to put in the air system, as in our New York house, or to put in a high brine system and have a large number of feet of pipe in each room. The latter system was adopted. We carry our eggs at 27° Fahr. in four rooms, and have plenty of brine circulation with as high a velocity as possible. All our pipes are placed on 30 feet centres; they are not on the walls, but hung vertically in the passageways. We feel that the eggs go out of there in better shape than they do in our New York house. Indirect circulation is all right for the storage of eggs, but requires a very delicate adjustment. If you have too high a velocity the eggs shrink; if you have too low they mould. With us, nobody goes near the rooms. We do not take the temperatures or bother with them from one end of the

season to the other. We do not change the air or have anything to do with the manipulation.

Louis Block.—How many eggs have you in storage?

V. R. H. Greene.—We have this year 375,000 cases.

Louis Block.—Last year?

V. R. H. Greene.—Considerably more. The exact quantity I do not know.

Louis Block.—I heard there were 600,000 cases.

Nelson J. Waite.—There are certain classes of goods stored in general cold storage warehouses where the air-circulation system is a decided disadvantage. One that I call to mind is the storage of dried fruit. Where you have air circulation there seems to be a very rapid evaporation and consequent shrinkage of the goods, of which the storer of the goods is liable to complain.

John E. Starr.—It seems to me that is the point that I was trying to make—that air circulation as usually applied, as a generic term, means rapid movement of the air. It does not follow at all that the movement of that air should be any more rapid than is desired or is requisite for the proper keeping of the goods. I have not the slightest doubt that in a great many air-circulation systems they have been used wrongly; that there has been too much air used, as we call it; that the circulation has been altogether too rapid. That is a point to be watched. But in the cold storage business it seems to me that as a rule vigilance is always necessary in every portion of the work, and a most happy condition of affairs must exist in an egg room which has not to receive at any time any attention at all.

Gardner T. Voorhees.—I want to say one word on the subject of watching. This impression, I think, will go out now that it is more difficult to regulate or watch the rooms with air circulation than it is for rooms directly piped; and I want to say from my experience that it is entirely the reverse. It seems to me it is much easier to go into a room and, if you please, take hold of a little piece of iron there and regulate that air damper than it is to regulate expansion valves or brine valves. It seems to me, from my experience, that the air system is much easier to regulate and it requires very little attention. All you have to do is to attend to your expansion valves or brine valves, and instead of running all over the building you have them right under your eyes in the bunker room.

Franklin H. Herst.—I would like to tell a little of my experience. I operate the air-circulation and the brine-circulation systems, and I agree with Mr. Starr in reference to the air circulation. In

storing fruits I have several rooms that hold 7,000 barrels of apples each; I have a series of ducts, one duct running right down through the centre of the room, a second series of ducts reaching across to the sides of the room, with slides at intervals along the ducts. Now, then, when we start to fill the rooms we run our speed up pretty well to circulate the air, and it is necessary at different times to adjust dampers and open slides, etc., in order to equalize the room. The piling of goods will certainly cause air currents, and it requires ducts all over the room in order to give proper distribution, and not create air currents. That is one of the reasons why there is trouble with air circulation. Now, then, after the room becomes cold and gets down to the proper temperature, I find that we can cut down the speed of the fan. As the goods get cold, and the proper temperature is arrived at some people make a mistake—they run a fan on a governor. I run the fan on valves and cut it down as the requirements come, and when the room becomes cold and equalized, which it will, by the series of valves and ducts and chambers, I can carry that room right through for six months without a variation of one or two degrees. I have customers in that same house that will take a brine room for apples instead of the air circulation, and then I have customers that want air circulation instead of brine circulation. But I firmly believe that when you use air circulation you must use a series of ducts and dampers, and regulate the currents of air as the temperature is required, and it is a great factor in the air circulation in the carrying of the rooms. It requires a good deal of attention to keep the proper air circulation at all times.

V. R. H. Greene.—I do not want to go on record as being opposed to the air-circulation system, but I think it does require more attention than the mere taking of temperatures. It is a question of the condition of the air. Mr. Voorhees just said that anybody can be trained to operate two bulb thermometers and figure out the percentage of moisture in the rooms; I do not know whether this is possible or not. In a big house, where there are a large number of men and superintendents, this might be done, but it is certainly impossible to trust the average temperature man to do anything like that.

Louis Block.—It is more expensive, is it not?

V. R. H. Greene.—I do not know.

Gardner T. Voorhees.—I would say it is less expensive.

Louis Block.—In building?

Gardner T. Voorhees.—Yes.

Louis Block.—In a house of a million cubic feet of space it

would cost $20,000 more to build that extra story on top for the cooling chamber.

Gardner T. Voorhees.—I think there is, however, one thing to remember when you have air circulation—that you do not lose space all around the sides of the room. If you have coils you have got to keep the goods a fair distance away from them.

Louis Block.—Not necessarily, if you have gutters under them.

Gardner T. Voorhees.—You keep your eggs away from the side.

Louis Block.—You always do that; never put them up to the sides.

V. R. H. Greene.—We have no side coils. We have coils hung through the two passageways. Our plant in Jersey City is designed for 1.8 B. T. U. per hour per degree difference, while the New York house, with air circulation, is piped for .6. Apparently the latter house is running satisfactorily, which means a saving in pipe of 3 to 1.

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* Associate Member.
§ Junior Member.
Names without reference marks are Members.
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